TRANSACTIONS

OF THE

NEW YORK ACADEMY OF SCIENCES

SERIES II

VOLUME 1 –11

NEW YORK
PUBLISHED BY THE ACADEMY
1938–1939

CONTENTS OF SERIES II, VOLUME 1

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 NOVEMBER, 1938 No. 1

The New York Academy of Sciences presents herewith the first issue of a new series of its historic TRANSACTIONS, as authorized by the Council of the Academy at its meeting of April 28, 1938.

The first series of the TRANSACTIONS was issued from 1881 to 1897. Its purpose was to print the scientific proceedings of the Academy meetings "regularly and promptly, during the sessions of the Academy, and to distribute them free to Resident Members and, in exchange for their publications, to prominent American educational institutions and scientific societies all over the world."[1]

After the TRANSACTIONS was terminated as a separate publication in the year 1897, the "Records of Meetings" were continued from time to time in parts of the *Annals* of the Academy, but finally ceased altogether, presumably due to increased cost of printing, the *Annals* thereafter, as at present, being devoted solely to scientific papers.

During the past few years, there has been an increasing desire on the part of many members to revive the TRANSACTIONS, as a separate series, to cover the proceedings of the stated meetings; various matters of general interest to the members of the Academy; and, especially, to give an opportunity to scientists presenting papers in the Sectional Meetings to publish abstracts of them at the earliest possible date. This latter feature would guarantee priority to new and original research, and increase the value of the TRANSACTIONS as a scientific record.

The Council takes this opportunity to send its greetings to you and requests your cooperation to make the TRANSACTIONS, Series II, a success.

[1] *A History of the New York Academy of Sciences*, by Herman LeRoy Fairchild, 1887, p. 123.

SECTION OF GEOLOGY AND MINERALOGY

October 3, 1938

Professor Reginald A. Daly, of Harvard University, delivered a lecture on *The Problem of Isostasy,* illustrated by lantern slides. No abstract was received.

The meeting of the Section of Geology and Mineralogy was preceded by a special Business Meeting of the Academy at which 20 candidates for Active Membership and 52 for the Associate Membership Class were elected. The following changes in the Constitution and By-Laws, as recommended by the Council, were unanimously adopted:

Changes in Constitution

That the first sentence of Article II of the Constitution be amended to read as follows:

"The Academy shall consist of *eight* classes of members, namely, *Sustaining Members,* Active Members, Fellows, Associate Members, Student Members, Affiliated Members, Corresponding Members and Honorary Members."

After "Honorary Members" insert:

"*Sustaining and* Active Members shall be members of the corporation who live in or near the City of New York . . . etc., as now stands to end of paragraph.

That Article III be amended to read as follows:

"None but Fellows, *Sustaining Members* and Active Members who have paid their dues up to and including the last fiscal year shall be entitled to vote or hold office in the Academy."

Amendments to By-Laws

Chapter IV

ELECTIONS

1. (a) The first paragraph to read as follows:

"*All Members except Affiliated Members* shall be nominated in writing to the Council by at least two *Members who are qualified to do so by right of full membership privileges.* If approved by the Council, they may be elected at the succeeding business meeting."

Chapter V

DUES

1. Dues. "*The annual dues of Sustaining Members shall be $10, payable in advance at the time of the Annual Meeting.*"

"The annual dues of Active Members shall be $5, payable in advance at the time of the Annual Meeting."

Chapter VI

PATRONS, DONORS AND LIFE MEMBERS

Paragraph 3 to read as follows:

3. Life Members. "Any Member or Fellow contributing at one time $100 to the general funds of the Academy shall be a Life Member and shall thereafter be exempt from annual dues; and, any *Sustaining Member* who has paid annual dues for twenty-five years or more may, upon his written request, be made a Life Member and be exempt from further payment of dues."

These changes will become effective January 1, 1939.

SECTION OF BIOLOGY

October 10, 1938

Doctor Ross F. Nigrelli, New York Aquarium: *Fish Parasites and Fish Diseases. 1. Tumors.* (This lecture was illustrated by lantern slides.)

Contrary to former beliefs, fish tumors are not rare and the majority of them, both in gross and histological appearances, agree in many respects with tumors of warm-blooded vertebrates. This is not surprising when one remembers the fundamental similarity in tissues and organs among all vertebrates. It should be expected that the histological structure and the normal and abnormal growth of tissues, should also be fundamentally the same. However, there are a few types of tumors that are peculiar to fishes.

As a result of the careful studies of Gaylord and Marsh (1914), Gordon (1931–1938), Haddow and Blake (1936), Johnstone (1926), Plehn (1906–1920), G. M. Smith and his collaborators (1936–1938), Takahashi (1929–1933), Thomas (1931), Williams (1929, 1930), etc., much has been done to stimulate interest in this field of pathology. As in human cancer studies, the cause of these abnormal growths is one of the main features of the present-day investigations. From these reports and studies made at the New York Aquarium, it has been found that the causative agents for the majority of the fish tumors have not been determined, but in certain cases tumor growths may be due to the following: (1) *Parasites.* Certain protozoa belonging to the Myxosporidia and Microsporidia will induce tumor formation in practically any organ or tissue of the body of marine and fresh-water fishes; (2) the adenocarcinoma of the thyroid of salmonoid fishes has been definitely shown to be due to a *chemical condition* of the water; (3) certain jaw tumors (osteomata) of trout kept in captivity are the result of *trauma;* (4) the melanoma (black pigmented tumors) found frequently in Mexican killifishes have been definitely shown to be the result of certain *genetic crosses;* (5) the

multiple lymphocystiosis of certain marine and fresh-water fishes has been reported to be due to an intracellular *virus*.

The following tumor types were encountered in fishes present in the New York Aquarium:

(1) *Myxosporidian tumors.* Nigrelli and Smith (1938) reported this type of tumor in several broad killifish (*Cyprinodon variegatus*). The growths are localized in the skin and, in a fish measuring 4.24 mm., they may reach a size of 1 cm. in diameter. Microscopically such tumors show a tremendous hypertrophy of fibroblastic cells, which form a delicate stroma supporting the parasites, the latter infiltrating muscle and bone, causing a degeneration of these tissues.

(2) *Lymphocystis.* This is a cutaneous disease in marine and fresh-water fishes characterized by the formation of small, solitary or confluent nodules of grayish-white color. Microscopically, these small tumors were found to be tremendously hypertrophied cells. Each enlarged cell is surrounded by a thick cellular membrane. The nucleus is also enlarged, and in larger cells may be completely degenerated. In young cells, the nucleus contains fine chromatin granules, a nucleolus and several nucleoli. The cytoplasm of these hypertrophied cells contains reticulated basophilic inclusions of various size and form. These inclusions differ in size, number, arrangement and compactness in the diseased cells of various fishes.

Earlier investigators indicated that these enlarged cells were some form of Cnidosporidia. It was Weissenberg (1914) who claimed that they were not protozoa, nor due to a protozoan infection, but instead they are hypertrophied connective tissue cells of the host. The normal connective tissue cells are very small, about 1/2000 of an inch, and in the diseased cells they may attain the tremendous size of 1/16 of an inch. Weissenberg (1920) further claimed to have induced tumor formation in healthy fishes by feeding them an emulsion of the diseased cells. From such an experiment, he concluded that the cells were stimulated to gigantic growth by some intracellular virus, basing this interpretation, in part, on the fact that

cell inclusions are present, which is a known characteristic for other and better known virus diseases.

Smith and Nigrelli (1937; 1938, unpublished data) have observed and described this disease from the following fishes present in the New York Aquarium: *Angelichthys isabelita* and *Lachnolaimus maximus* from Florida; *Ceratancanthus schoepfi* from Sandy Hook Bay, N. J., and *Amphiprion percula* from the East Indies. The disease makes its appearance in late spring, reaching a maximum intensity during the summer, gradually decreasing and finally disappearing in fall and winter. It was further observed that other species of fishes present in the tanks with infected fish never become diseased. Attempts to transfer the disease to killifish (*Fundulus heteroclitus*) by feeding and injecting lymphocystis material have thus far been unsuccessful. In all the tissues examined no parasites were found.

The myxosporidian tumor reported above can be considered, in certain respects, a sarcoma, for here we truly have a stimulation of growth of connective tissue cells. Tumors due to intracellular parasites (*e.g.,* certain microsporidian diseases) or virus (?) are more difficult to classify. If the growth had taken place in the form of a rapid multiplication of the cells involved, the size remaining the same, we would have a true sarcoma. But in these tumors, the infected cells grow in size and not in number.

The mammalian-like tumors found in fishes have been classified according to the type of tissue involved. Benign tumors of connective tissue character that have been reported are fibroma, lipoma, chrondroma and osteoma. Benign tumors of striated muscle (myoma), nerve cells (neuroma) and blood channels (hæmangioma) also have been reported in certain species of fishes. The malignant tumors of connective tissue character described are round cell sarcoma, liposarcoma, myxosarcoma, osteosarcoma, lymphosarcoma, melanoma, etc.

Neoplastic growths that have their origin in epithelial tissues are rarer in fishes than they are in mammals. Those that

have been reported were usually benign or remained localized in the region of origin, with some tendency to infiltrate adjacent tissue. Most of these tumors occurred in the skin and gave rise to a hyperplasia of the dermal epithelium, producing papilloma or in certain instances more malignant epithelioma. Adenoma or glandular tumors are even rarer in fishes than epithelial tumors of the skin.

The following tumor types have been described from fishes present in the New York Aquarium:

(1) Sarcoma. (a) Melanoma in the skin of the lungfish (Smith and Coates, 1936) and hybrid Mexican killifish (Smith, Coates and Strong, 1936); (b) Erythrophoroma, or red pigmented tumor, also in hybrid Mexican killifish (Smith, Coates and Strong, 1936); (c) fibroma in the skin of *Rasbora daniconus* (Smith, Coates and Strong, 1936); (d) lymphosarcoma in the body cavity of *Rasbora lateristriata* (Smith, Coates and Strong, 1936); (e) osteomata on the jaws of trout and salmon (Smith and Nigrelli, unpublished data).

(2) Tumor of epithelial character. (a) Adenocarcinoma of the thyroid of *Rasbora lateristriata, Heterandria formosa* (Smith, Coates and Strong, 1936; Smith and Coates, 1937), and brook trout (Nigrelli, unpublished data); (b) papilloma on skin of electric eel, *Electrophorus electricus* (Coates, Cox and Smith, 1938); (c) "pox" disease on the skin of blue-gilled sunfish (Smith and Nigrelli, unpublished data).

From studies on fish tumors certain conclusions can be made as to their characteristics. (1) The majority of the growths have the various connective tissues of the body for their origin and are most often found in the skin. (2) Only a rare few are derived from epithelial cells. (3) The rate of growth is comparatively slow. (4) In the case of a malignant growth, either sarcoma or carcinoma, there is a general tendency to remain localized in the region where it is started. Although there may be a certain amount of infiltration into adjacent tissues, the production of secondary growth which is frequently found in bird and mammalian tumors occurs only rarely in fishes (*e.g.*, adenocarcinoma of trout).

SECTION OF PSYCHOLOGY

October 17, 1938

Professor L. E. Wiley, Ohio Wesleyan University: *The Learning Function.*

The purpose of this study is to test experimentally the theoretical learning curve of Mr. Thurstone and to derive therefrom an adequate measure of learning.

Five mazes of the Lashley-type were used. In this type of maze the number of blind alleys may be increased indefinitely without making any fundamental change in the plan of the maze. A 16-cul-de-sac maze was built in such a manner that it could be broken up into smaller mazes of 4, 8, or 12 culs-de-sac, thus making four mazes that differed only in the number of blind alleys. These four mazes are designated as the variable mazes. A fifth maze of 8 blinds was used and is referred to as the constant maze. The general plan of Maze V was similar to the group of variable mazes except that the order of turns was reversed and the relative positions of the food box and the starting box were altered.

One hundred and twenty-seven rats with varying amounts of brain lesion and sixty normal animals were trained. They were divided into four groups, approximately equal as to the size and locus of cerebral lesion. Each group was trained on one of the variable mazes and on Maze V, the constant maze. They were trained on the variable maze first.

The animals were selected at random from the general colony. They were given preliminary training in a straight runway, ten feet long, until they ran the runway promptly and were no longer disturbed by handling. They were trained until they made ten consecutive errorless runs. If they ran 150 trials in the variable maze without reaching the criterion training was stopped. Training in the constant maze was stopped after 100 trials.

After training, the brains were removed and reconstructed according to the techniques developed by Lashley. The theo-

retical learning curve was fit to the learning records of each animal.

The learning curve is of the form

$$u = \frac{\sqrt{m}}{ak} - \frac{\sqrt{m}}{k} \cdot \frac{u}{R}, \tag{1}$$

where u represents the accumulated errors, R is the number of trials, k the learning constant of the animal, m the difficulty of the maze and a is an arbitrary constant that can be absorbed in m. In our analysis the curve appears in the form

$$u' = A + \frac{BR'}{C + R'}, \tag{2}$$

where A, B, and C are constants. Thurstone's curve represents the situation in which the origin of the system of coordinates lies at the point where *learning* begins. The form used in this experiment measures the variables from the point where *training* begins.

The curve is an equilateral hyperbola. The semi-major axis may be used, therefore, to describe the entire curve. At the vertex all of the animals eliminate errors at the rate of one error for each trial.

The following critical values of the curve were computed for the variable mazes and for the constant maze:

1. The horizontal asymptote.
2. The length of the semi-major axis.
3. Trials at the vertex.
4. Errors at the vertex.

The relationships between the determinations of the critical values were found. Each critical value was related to the amount of cerebral cortical tissue removed. Since the animals were trained on two learning situations it was possible to separate the learning ability, k, and the difficulty of the problem, m. K was compared with the artificially induced individual differences as measured by the lesion. M was compared with the increasing difficulty of the problem as measured by the number of culs-de-sac.

From the experimental results we draw the following conclusions:

1. Professor Thurstone's theoretical learning curve fits data collected on the Lashley-type maze since the horizontal asymptote gives linear relationships with the experimentally determined limit of learning, total-errors-minus-errors-on-the-first-trial.

2. The method proposed is internally consistent. That the critical values measure the same attribute of learning in both situations is shown by the linear relationships existing between the two determinations.

   a. The horizontal asymptote calculated from the variable mazes gives a linear relationship with the horizontal asymptote obtained from the constant maze for the group of animals with brain operations.

   b. The semi-major axis obtained from the variable mazes is in a linear relationship with the semi-major axis from the constant maze for all animals.

   c. Both trials and errors at the vertex show definite linear relationships from one maze to the other for the group with brain lesion.

3. That the critical values may be considered adequate measures of ability is shown by the relationships existing between them and the extent of cerebral cortical lesion. All of these are continuous curvilinear functions.

   a. As the extent of brain lesion increases the horizontal asymptote increases.

   b. As the extent of brain lesion increases both trials and errors at the vertex increase.

   c. As the extent of brain lesion increases the semi-major axis increases.

4. That the learning ability in the theoretical curve is a measure of the ability to learn is shown by its relationship to artificially produced individual differences as measured by the extent of cerebral cortical lesion. As the lesion increases the learning ability, K, decreases in a curvilinear function.

5. That the difficulty of the problem, m, is a measure of the difficulty of the learning situation is shown by the computed values, m, which increase as the number of culs-de-sac in the problem is increased for both the normal animals and the animals with brain lesion.

Upon the evidence presented we conclude that Professor Thurstone's theoretical learning curve, used according to the methods developed in this experiment, is an adequate description of the learning process since it fits learning data and since the constants of the curve behave in a manner compatible with their definition.

We wish to acknowledge a grant-in-aid from the National Research Council which in part financed this study.

L. E. Wiley
A. M. Wiley

Ohio Wesleyan University
Delaware, Ohio

SECTION OF ANTHROPOLOGY

October 24, 1938

Doctors William Walter Greulich and Herbert Thoms, Yale University: *A Study of Pelvic Type in White Women.* (This lecture was illustrated by lantern slides.)

The size and the shape of the pelvic inlet were determined by means of Thoms' method of X-ray pelvimetry on 582 primigravid white women from the Obstetrical Clinic of the New Haven Hospital, 104 nulliparous young student nurses from a somewhat different racial stock and a much more privileged economic class than the clinic women, and 107 young girls who ranged in age from 5 to 15 years. The pelves of this group were classified according to Thoms' system of classification, which is based on the relative size of the antero-posterior and the maximum transverse diameters of the pelvic inlet and which distinguishes the following pelvic types:

Dolichopellic: The antero-posterior diameter of the inlet exceeds the maximum transverse diameter.

Mesatipellic: The maximum transverse diameter either equals the antero-posterior diameter or exceeds it by no more than 1 cm.

Brachypellic: The maximum transverse diameter exceeds the antero-posterior diameter by from 1.1 to 2.9 cm.

Platypellic: The maximum transverse diameter exceeds the antero-posterior by 3 cm. or more.

The observed incidence of the various types of pelves, according to this classification, among the 686 adult women of our series, was:

Type	*582 clinic women*	*104 student nurses*	*Total 686 adult*
	%	%	%
Dolichopellic	15.0	37.5	18.4
Mesatipellic	44.8	44.2	44.7
Brachypellic	34.3	18.2	31.8
Platypellic	5.6	....	4.7

For the past 150 years the "normal" female pelvis has been described in textbooks of anatomy and of obstetrics as one in

which the transverse diameter of the inlet exceeds the anteroposterior, or conjugate diameter, by more than 2 cm. This type of pelvis was found in only 14.9% of the clinic women and in only 5.7% of the student nurses. It may be added parenthetically that only 31.9% of the 686 adults of this series had the type of pelvis which, according to the anthropological literature, is proper for white women. It appears, therefore, that the prevailing concept of the "normal" pelvis of white women is in need of some revision.

In an attempt to investigate what might be called the functional adequacy of various types of pelves, the case records of 600 white primipara from the same clinic were reviewed and the percentage of women of each pelvic type who required some kind of operative intervention during labor was determined. As "operative intervention" were included, not only Caesarean sections, but also version extractions, midplane forceps, and outlet forceps deliveries. The incidence of operative intervention, as thus defined, was lowest among the Dolichopellic, somewhat higher among the Mesatipellic, still higher among the Brachypellic, and highest among the Platypellic. The incidence of Caesarean section in this same series was 0.0% for the Dolichopellic, .0.7% for the Mesatipellic, 4.3% for the Brachypellic, and 15.4% for the Platypellic group. If the proof of the adequacy of a given pelvic type is its ability to function efficiently during parturition, the adequacy of the long oval (Dolichopellic) and of the round (Mesatipellic) pelves appears to be convincingly demonstrated by these findings. There was certainly no evidence of any functional superiority of the pelvis with the transversely elongated inlet—the type which is usually regarded as "normal."

A comparison of body build and pelvic type in 132 of the clinic women and the 104 student nurses disclosed the following relationships: (a) The women with long oval pelves were predominantly tall, long-headed, and broad-shouldered; the width of their pelvis between the iliac crests and of their hips between the trochanters was smaller in proportion to the width

of their shoulders than in women of the other pelvic types; and they had the largest average external conjugate diameter. (b) The women with transversely elongated pelves were, on the average, the shortest of the series and they had the broadest heads, the narrowest shoulders, and widest pelves and hips in proportion to the width of their shoulders, and the smallest average external conjugate diameter. (c) The women with round pelves were approximately intermediate between the other two groups in all of these dimensions. There was, however, so much variation in these external dimensions between student nurses and clinic women of the same pelvic type that it would be very hazardous to attempt to predict pelvic type on the basis of those dimensions, in individual cases, in a population as heterogeneous as that of this country.

The high incidence of long oval and of round pelves among the largest women of both groups suggests the possibility that factors which make for the attainment of maximum normal growth tend to prevent that degree of antero-posterior flattening of the pelvis which has come to be regarded as characteristically feminine.

A comparison of photographs, external measurements and pelvic tracings of 132 clinic women and the 107 student nurses showed that some women who resembled each other very closely in size and in general body build had pelves which were very dissimilar and that, conversely, women in whom the size and shape of the pelvic inlet were identical resembled each other rather closely, only slightly, or not at all in general appearance and in external bodily dimensions.

These findings indicate that the type of pelvic inlet can be determined in the intact, living woman only by the use of some dependable method of roentgen pelvimetry. It cannot with certainty be deduced from external measurements, and the ability to infer it correctly from general body build would appear to be one of the more esoteric of anthropological accomplishments.

SECTION OF PHYSICS AND CHEMISTRY

October 28 and 29, 1938

Conference on "Electrophoresis"

The new section of Physics and Chemistry started with a "Conference on Electrophoresis," the first of a projected series.

It can be said without reserve that this initial conference was a success, both in the attendance and in the enthusiasm shown during and after the meeting. The ideas underlying the organization of the conference were threefold. A topic for discussion engaging the interest of active workers was chosen. A short program of papers was arranged, allowing for quite as much time for discussion as for presentation. Attendance was limited to those invited and to members of the Academy. Possibly half of those present (totaling about fifty) came from outside of New York City. Visitors from Washington, Ithaca, Boston, Cambridge, New Haven and Schenectady attended. The program consisted of papers on the theory of electrophoretic migration by Professor Hans Müller; on the microscopic and moving boundary methods for studying electrophoresis by Doctors H. A. Abramson and K. G. Stern, respectively; the optical and other properties of electrophoretic boundaries by Doctor L. G. Longsworth, and a paper by Professor E. J. Cohn on the effects of ionic strength and pH on electrophoretic mobility. The discussion of all these papers was animated and penetrating.

Two other conferences are being arranged for the present academic year. These will be on the topics "Reaction Kinetics in Solution" and "Dielectrics," which will be under the guidance of Professor V. K. La Mer and Professor C. P. Smyth.

Duncan A. McInnes, *Chairman*

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 1, No. 1, November, 1938.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Editor: Eunice Thomas Miner, Executive Secretary.

Application for entry as Second-class Matter is pending.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 DECEMBER, 1938 No. 2

SECTION OF GEOLOGY AND MINERALOGY

NOVEMBER 7, 1938

PROFESSOR CHESTER R. LONGWELL, Yale University: *The Basin Range Problem.* (This lecture was illustrated by lantern slides.)

The term *Basin Range System* was first proposed by Gilbert, to designate the "orographic province which has its type in the Great Basin, but is not coincident with it."[1] He recognized that mountain ranges of one general type, structurally and physiographically, occupy the vast area of interior drainage between the Wasatch Mountains and the Sierra Nevada, and additional large areas with interior drainage extending far to the south and southeast. Gilbert was the first also to pose the fundamental problem presented by this system of ranges, when he stated that "the movements of the strata by which ridges have been produced have been in chief part vertical along planes of fracture, and have not involved great horizontal compression."[2] This concept, new in geologic thought, appeared to contradict the views of Clarence King who, in his report of the surveys along the fortieth parallel (1867–1877),

[1] Gilbert, G. K., *Report upon geographical and geological explorations and surveys west of the one-hundredth meridian*, vol. 3, p. 22, 1875.

[2] *Idem*, p. 42.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 1, No. 2, December, 1938.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

sought to explain the ranges of central Nevada and western Utah as greatly eroded anticlines.

Powell suggested a history of the Basin Ranges in which both King's and Gilbert's concepts find a logical place. Folding of the region was succeeded by a long interval during which the surface was nearly levelled by erosion. Block-faulting then fashioned the ranges and basins in their present form, except for modification by later erosion and deposition.[3] Essentially this view has been widely accepted. However, fault-block mountains were not recognized as an important class in other parts of the world for many years after Powell included the "Basin Range Type" in his systematic classification of mountains. As late as 1896, LeConte commented that the Basin Ranges might be the only representatives of their class.[4]

W. M. Davis found the Basin Ranges a fascinating subject, and wrote several papers in which he supported the views of Gilbert and Powell.[5] To assign proper credit for recognition of the major stages in Basin Range development, Davis referred to the "King mountains," formed by folding in mid-Mesozoic time; the "Powell surface," developed during the late Mesozoic and early Tertiary; and the "Gilbert blocks," representing large-scale block-faulting in late Cenozoic time. Later, on learning that important compressive deformation affected formations as young as mid-Tertiary, Davis commented that discovery of these younger structural features does not invalidate his general scheme, but merely adds one or more generations of fold mountains to the stage preceding development of the "Powell surface."

With continued field investigation, the known history of the Basin and Range province grows ever more complex. Three major episodes of compressive deformation are now recog-

[3] Powell, J. W., *Report on the geology of the eastern portion of the Uinta Mts. and a region adjacent thereto*, pp. 32–34, 1876.

[4] LeConte, J., *Elements of geology*, 4th edition, p. 277, 1896.

[5] See, among others, *The mountain ranges of the Great Basin*, Bull. Mus. Comp. Zool., Geol. Ser., vol. 6, no. 3, pp. 129–177, 1903; *The Basin Range problem*, Proc. Nat. Acad. Sci., vol. 11, no. 7, pp. 387–392, 1925; *Basin Range types*, Science, vol. 76, pp. 241–245, 1932.

nized: The first, which occurred near the end of the Jurassic period, is recorded in close folds and large thrusts in the ranges of western Nevada; the second, dated as "Laramide," caused gigantic thrust faults in the eastern part of the province; the third, post-Miocene in date, formed equally impressive thrust faults in a wide belt running through eastern California and western Nevada. Block faulting on a large scale began before some of the great thrusts developed, and has been continuous or recurrent in parts of the region until recent time. Thus the three major stages suggested by Davis are not clearly separable, unless thrust faulting entered into the formation of the "Gilbert blocks."

The Basin Range problem is both geomorphic and structural. Gilbert and Davis were concerned primarily with the origin of the ranges as surface forms; and their major point—that most of the ranges owe their existence to movements on large faults—is becoming more firmly established as field evidence accumulates. The structural problem consists in determining the kinds and age relations of the structural features, and construction of a plausible synthesis relating these features to regional forces. Since a large part of the region is still to be mapped geologically, a geometrical picture of the province can be constructed only by extrapolation from scattered sample areas—an extremely dangerous practise in a district of complicated geology. Field work now in progress in the Death Valley area is bringing to light major structural features hitherto unsuspected. Any attempted synthesis based on the incomplete data now in hand must be highly speculative. Some of the major questions and suggestions that may be ventured at present are as follows:

1. Late Tertiary structural features of the region speak eloquently of compressive as well as tensional stresses. Has there been alternate application of these two types of stress to the entire region, or have tensional stresses developed locally during regional compression?

2. Some of the major tectonic lines are strikingly parallel

to the great active shear zones in California. Were the Basin Range faults developed in connection with movements along northwesterly shear zones?

3. During the Tertiary there was colossal volcanism in the Basin Range province and adjacent regions. Was this igneous activity merely a response to the large-scale faulting, or did it reflect subcrustal conditions that favored the crustal break-up?

LaConte's simple concept of a collapsed arch hundreds of miles wide now has few if any adherents. However, the "intumescent lavas" of his hypothesis may have been an important factor in weakening the crust, especially in the Great Basin. The systematic pattern made by the ranges in the entire province suggests a yielding to tectonic stresses that were similar in a remarkably wide region. However, it does not seem possible to explain the latest thrusts, the steep "Basin Range" faults, and the tilting of range blocks as results of the same regional forces.

SECTION OF BIOLOGY

NOVEMBER 14, 1938

DOCTOR WENDELL M. STANLEY, Rockefeller Institute for Medical Research, Princeton, N. J.: *The Nature of Viruses.* (This lecture was illustrated by lantern slides.)

In 1892 Iwanowski discovered that the juice from plants diseased with tobacco mosaic remained infectious after being passed through a Chamberland filter, a device that was considered to hold back all known bacteria or living organisms. Despite the fact that he was unable to demonstrate the presence of bacteria in the infectious filtrate, he concluded that the disease was bacterial in nature. Six years later Beijerinck repeated and extended the filtration experiments and concluded that the disease was not caused by ordinary bacteria but by a "contagium vivum fluidum." Thus, although Iwanowski was the first to demonstrate the existence of one of the group of infectious disease-producing agents that we now call viruses, Beijerinck was the first to recognize the true significance of the results and the fact that viruses differ from ordinary bacteria. Although the property of filterability was the earliest recognized property of viruses and has continued to be considered generally as characteristic of viruses, it should be noted that it has not been found an infallible means of distinguishing between viruses and bacteria. Today viruses are recognized not by the sole means of their filterability but by means of a set of general properties that emphasize not only their small size and the fact that they may change or mutate, but especially the intimate relationship that exists between viruses and their host cells. This relationship is manifested by the fact that viruses grow or multiply only within the cells of certain hosts and have never been found to grow on cell-free media, the fact that many virus-infected cells contain inclusion bodies, and the fact that a host recovered from a virus disease is usually, but not always, immune from a second attack of the

same or related virus. Since some of the properties that are used to characterize viruses, such as their reproduction, mutation, and specificity of action, have been regarded for years as being characteristic of living organisms, there has been a general tendency to regard viruses as submicroscopic living organisms somewhat similar to the bacteria.

Early in 1935 there was isolated from mosaic-diseased Turkish tobacco plants an unusual high molecular weight nucleoprotein possessing the properties of tobacco mosaic virus. The same protein was isolated from many different batches of diseased Turkish tobacco plants or from other mosaic-diseased plants such as tomato, common nightshade, petunia, spinach, or phlox. It was impossible to demonstrate the presence of material other than virus protein in purified preparations even by the sensitive precipitin or anaphylactic reactions. The protein was found to be highly active as a precipitinogen, but only weakly anaphylactogenic when tested *in vivo* and inactive when tested *in vitro* by the Schultz-Dale technique. The protein was found to be homogeneous in electrochemical sense and with respect to sedimentation constant. The ultraviolet absorption spectrum was found to agree essentially with the destruction spectrum of virus activity. Denaturation of virus protein preparations by any of several procedures always resulted in a corresponding loss or decrease in activity. At hydrogen ion concentrations more acid than about pH 2 or more alkaline than about pH 8, the protein was split into components of lower molecular weight with loss of virus activity. It was found impossible to separate the virus activity from the protein by any one of several different procedures, such as by filtration through collodion or other types of filters or by centrifugation of the protein from solution under a variety of conditions. Treatment of the virus protein with formaldehyde caused loss of activity, a decrease in amino nitrogen, and a decrease in the color developed by Folin's tyrosine reagent. It was found possible to reactivate the virus to a considerable extent and to demonstrate that the

reactivation was accompanied by an increase in amino nitrogen and in the color developed with the tyrosine reagent. The molecules of the protein are regarded as being rod-shaped because concentrated solutions are liquid crystalline and because dilute solutions show positive stream and electrical double refraction and the Ganz depolarization effect. The results of ultra-filtration and X-ray diffraction studies and various combinations of viscosity, sedimentation, stream double refraction, and diffusion data indicate that the molecules have a molecular weight of at least 43 millions, a length of at least 430 mμ, and a diameter of about 12 mμ. The English workers regard the purified virus protein to consist of aggregates of smaller units present in untreated preparations and have stressed the fact that on purification their preparations lost activity and the ability to pass membranes of average pore size, 450 mμ, and gave an increased amount of stream double refraction. However, it has been found in the Institute laboratories that the protein isolated by careful ultra-centrifugation is essentially the same as the protein as it exists in the plant juice. The virus activity, sedimentation constant, filterability, and stream double refraction of virus protein in juice and following purification were found to be the same. The results indicate not only that the virus protein can be isolated in an unchanged condition but that the protein isolated is tobacco mosaic virus.

Since the isolation of tobacco mosaic virus, over a dozen different specific and highly characteristic macromolecular proteins possessing the properties of the respective viruses or virus strains have been isolated. Although these materials have not been studied as extensively as tobacco mosaic virus, there is no reason at the present time to believe that they differ fundamentally. It becomes necessary, therefore, to consider virus activity in terms of these proteins. However, certain difficulties arise, for the virus proteins have the properties of molecules, whereas virus activity, involving reproduction, mutation and specificity of action, has been generally regarded

as a property of living organisms. At present at least two theories have been proposed to explain virus activity and this apparent discrepancy. According to one, the cell thory of life would be discarded and the virus proteins accepted as a special type of elementary living organisms, whereas, according to the other, the virus proteins would be regarded not as living but as autocatalysts of the type of pepsin and trypsin. The advantages and disadvantages of the two theories were discussed, and it was pointed out that, although the latter theory does not fit the experimental facts in that neither the existence of a precursor nor the production of a virus *de novo* has been demonstrated, additional work may eliminate these objections. The possibility that viruses may be elementary living organisms arising either by evolution or by retrograde evolution was discussed. It was pointed out that a molecule might have sufficient structure to endow it with unusual properties which might not interfere with its properties as a molecule but which might permit it to function as an organism when in the presence of a certain type of protoplasm.

SECTION OF PSYCHOLOGY

NOVEMBER 21, 1938

DOCTOR TRIGANT BURROW, The Lifwynn Foundation: *An Organismic Study of Behavior in the Individual and in the Community.*

There is a noteworthy feature that differentiates man's approach to his own behavior or motivation as contrasted with his approach to the behavior or motivation observable in other forms of life and to the phenomena of the external environment generally. Due to the adoption of controlled scientific methods in his relation to the outer environment, man has learned increasingly to maintain a clear-cut precision in respect to the data relating to the external management of things. In all our dealings with the reactions of animals and their motivation, in our study of plants and of minerals, our procedure is unfailingly objective and experimental. But with regard to processes that are internal to man—processes that motivate and direct the behavior of man's own organism as a species or phylum—man has failed to apply objective measures which would lead to similar progress. In our dealing with the behavior of man and his motivation our attitude is not experimental and objective but it is invariably prepossessive and traditional.

This direct relation of man to his outer circumstance or environment exists no less in the sphere of medicine as it applies to physical disorders objectively observable within man himself. Where inept reactions have occurred as a result of disharmony, disease or *dis*behavior in the outer environment, man has learned increasingly to apply immediate objective remedies to their adjustment or alleviation. But where there has occurred inept behavior, or disbehavior, in man's relation to man, where disorder has occurred in the relationships of men to one another subjectively, we have not only not applied the suitable objective remedies but instead the ineptness and disbehavior has become the accepted norm. Through the

force of sheer subjective habit, conflict has prevailed over man's processes, and disbehavior has displaced his basic behavior-reactions. The evidence of this widespread disharmony is seen in the prevalence of crime, war, industrial conflict, domestic infelicities, political, economic and religious discord. It would appear, then, that through some *faux pas* in his development man's behavior bears the stamp of a maladaptation or disbehavior in respect to himself that has superseded the primary reaction of his organism in relation to the immediate environment.

This evolutionary *faux pas* has to do with complications that have arisen in the relation of the symbolic segment to the organism as a whole. For the central configuration that presides over and preserves the consistency of the organism's symbolic field of perception on the one hand, and the central configuration that preserves the organism's world of internal feeling on the other have become entangled and confused in the pursuance of their appropriate objectives because of the too close approximation of these two major reaction-spheres to one another. In the work of my associates and myself our endeavor has been precisely to bring to book this confusion or overlapping between the organism's total feeling-reactions and the behavior based upon its external perceptual or symbolic code of reactions. Our interest in this challenge has been a preeminently practical one. For researches conducted over a good many years on the interreactions of individuals and groups afford unmistakable evidence that the very kernel of man's behavior-disorders, individual and social, are traceable to this confusion or conflict between spheres of the organism's motivation which require for their healthy functioning that they operate in definite coordination with each other.

Some eighteen years ago several of my associates, a number of my students and myself entered upon an experiment in group-analysis and in group-living which had as its purpose the blocking and frustration of such behavior-expressions as resulted from the overlapping and confusion of these two

spheres. It was not an easy job. Affective responses and behavior inconsistencies that are glossed over socially or mutually accepted by the participants came in for stern challenge. As a social group we undertook to challenge and defeat our accustomed level of interrelationships. The disparagement of these relationships had the effect of forcing the interest or preoccupation away from these socially current affectivities and of directing it instead toward the internal sensations that pertain to the organism's immediate behavior reaction. Instead of being occupied with the behavior of ourselves and of others we became aware of and interested in a physiological conflict in tensional patterns which accompanied the affecto-symbolic responses. Our aim, therefore, was henceforward directed toward the clear organization of these internally appreciable sensations.

This task was accomplished in part at least by intercepting the accustomed focus of attention upon the separate item or image as it occurs in ordinary thinking and, instead, allowing the sensations associated with the process of mentation to become perceptible in the internal neuromuscular reactions that accompany this activity. In this process of turning to these internal tensional patterns—a process called *cotention*—it was possible to discriminate between patterns related to man's conditioned part-functions and patterns related to man's basic adaptation. A physiological feature which was observed during periods of cotention consisted in a markedly slowed respiratory rhythm. In order to obtain more accurate measurements of these respiratory modifications, records were taken of the thoracic and abdominal respiratory curves by means of a wax paper kymograph devised by Volkmann and Gerbrands of the Harvard Psychological Laboratory. The diminution in frequency was from a normal average of 12 to 16 to an average of 3 to 6 respirations per minute during cotention. The alterations in the respiratory curve were typical and consistent in different experimental settings. It was possible to differen-

tiate these alterations sharply from slight modifications occurring under a variety of experimental situations.

It will be seen that, in phylobiological studies of human behavior undertaken by my associates and myself, we have attempted to view the problem of man's adjustment to man—neurosis, crime, social and economic disorders—wholly apart from the ideological prepossessions that prevail generally in respect to the obvious outer appearances of "mental" disorder. In having recourse to a frame of reference which regards disorders of behavior not as isolated disease-entities but as phyletic manifestations there would seem to be opened the possibility of a more consistent approach to the field of human conflict.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 | January, 1939 | No. 3

SECTION OF GEOLOGY AND MINERALOGY

December 5, 1938

Doctor Wallace W. Atwood, President, Clark University: *The Late Physical History of the Rocky Mountains in the United States.* (This lecture was illustrated by lantern slides.)

The major structural features of the Rocky Mountains are due to the great physical revolution which closed the Mesozoic and opened the Cenozoic eras in that part of the world. In the Rocky Mountain area the early Eocene landscape must have included a number of domes and of anticlinal arches that were rising slowly above the general level of an ancient sea bottom.

As mountain growth continued stream erosion became more and more vigorous. The uplifted rock masses were carved into bold relief features and among the summits of the higher ranges alpine glaciers formed. Before the close of the Eocene period certain broad erosion surfaces were produced and the lower lands bordering the mountain ranges were built up by the deposition of sediments that came from the higher lands. During the Eocene period there had been some vulcanism, and with the renewal of mountain growth near the close of Eocene time vulcanism became very conspicuous at many localities.

TRANSACTIONS of the New York Academy of Sciences, Series II, Volume 1, No. 3, January, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

There then followed a long mid-Tertiary period of erosion and basin filling, during which the one widespread notable peneplain surface of the Rocky Mountain region was developed. The remnants of that surface are here referred to as parts of the Rocky Mountain Peneplain. The geologic clock had moved on to late Tertiary time. Much of the landscape was subdued; picturesque remnants of the mountain ranges still rose above the great peneplain as monadnocks; there were vast expanses of old age erosion topography and still more extensive waste-filled basins. Much of the mountain structure in the ranges and between the ranges was buried beneath the alluvial filling which had accumulated as the mountains were being worn away.

The long period of stability when the Rocky Mountain Peneplain was developed was brought to a close by a widespread epirogenic uplift. Streams were rejuvenated and a new cycle of erosion was inaugurated. During this period many of the major streams found themselves superimposed upon buried mountain ranges, and as they lowered their courses the great gorges, water gaps, and gateways that add much of scenic attractiveness to the Rocky Mountains were excavated. At this time the Black Canyon of the Gunnison, the gorges of the North Platte and the Laramie were cut. The canyons of the Green River and of the Wind River were excavated. The Royal Gorge of the Arkansas, the Canyons of the Big Horn, and a score of other notches were carved through uplifted mountain masses.

The cycle of erosion during which streams were uncovering the buried ranges and cutting many canyons and gorges was interrupted by a formation of Pleistocene ice. At least three stages of glaciation are recorded during Pleistocene time in these mountains. Since the disappearance of most of that ice the streams have renewed their work of uncovering mountain ranges and of deepening their valleys.

All of the major events that have punctuated the late physical history of the Rocky Mountain region are expressed in one

way or another in each of the ranges, and must be recorded in a complementary way in the sediments of the basin-like areas among the mountains and in the Great Plains province to the east.

SECTION OF BIOLOGY

DECEMBER 12, 1938

DOCTOR RAYMOND C. PARKER, Rockefeller Institute for Medical Research: *Some Recent Advances in Tissue Culture.* (This lecture was illustrated by lantern slides and motion pictures.) No abstract was received.

REPORT OF THE ANNUAL MEETING

DECEMBER 14, 1938

The 120th Annual Meeting of the Academy for the election of Officers and Fellows, the presentation of reports and the transaction of other business was held at The Hotel Astor on the evening of Wednesday, December 14th.

The Corresponding Secretary reported that there are now upon the rolls of the Academy 44 Honorary Members and 7 Corresponding Members. Two deaths were reported during the past year.

The Recording Secretary reported that, during the year 1938, the Academy held 7 Business Meetings, 1 Round Table Conference, and 26 Sectional Meetings, at which 31 stated papers were presented. Besides the regular Annual Dinner, one General Meeting was held in conjunction with the New York Mineralogical Club and The American Museum of Natural History, at which approximately 1,000 persons were present. The newly formed Section of Physics and Chemistry held a two-day Conference on the subject of "Electrophoresis," which marked a new step in the progressive activities of the Academy, and was not only successful, but was enthusiastically received by the research workers in this highly specialized field. Four smokers were held under the auspices of the various Sections of the Academy with a gratifying attendance at each.

The Executive Committee wishes to acknowledge the cooperation of the Works Progress Administration in furnishing the services of two secretarial assistants to the Academy office.

One Life Member, 3 Sustaining Members, 86 Active Members, 4 Student Members and 116 Associate Members were added to the rolls. Thus a total of 210 new members were added during the year.

The membership of the Academy is at present 747, which includes 2 Patrons, 93 Life Members, 78 Annual Sustaining

Members, 299 Annual Active Members, 80 Student Members, 143 Associate Members, 44 Honorary Members and 7 Corresponding Members; of these 192 are Fellows.

The Academy has lost by death 1 Patron, 3 Life Members, 10 Active Members, 1 Associate Member and 2 Honorary Members.

The Editor reported that four parts of the Annals have appeared, completing Volume 37, and that another part, completing Volume 38, was in press and undoubtedly would be ready for distribution before the end of the calendar year.

Volume VIII, Part 3, of The Scientific Survey of Porto Rico and the Virgin Islands, will also be printed before January 1, 1939. Two additional parts of the Scientific Survey have been edited, which will be ready for distribution before May 1, 1939.

Beginning with the October meetings of this year, the Academy has re-ëstablished the printing of its TRANSACTIONS, and Series II, Volume 1, No. 1, has been sent to all Members of the Academy, and to its exchanges. In this Series will be printed abstracts of papers presented at Academy meetings, as well as Academy notices of importance to Members.

The Librarian reported that 619 separate parts of the Annals have been distributed to the membership list and authors gratis. Twelve hundred separate parts, as well as one complete set of TRANSACTIONS, Series I; Proceedings; and, Annals of the Lyceum and Academy have been sold during the past year to non-members and authors.

During the year 1938, by authority of the Council, 127 parts of The Scientific Survey of Porto Rico and the Virgin Islands were sent to Members, upon payment of their dues. In addition, 120 parts were sold although no new publications in this series appeared this year.

The Library of the Academy sent out 1,585 copies of the Annals to its exchange institutions, and has received in turn 2,328 publications.

An inventory is being made of all publications in stock and will be completed during the coming year.

The Treasurer reported that the surplus for the fiscal year ending November 30, 1937, was $11,425.47. During the past year ending November 30, 1938, receipts from all sources amounted to $14,896.18, making a total with the amount transferred above of $26,321.45. The disbursments for the past year amounted to $11,559.65, leaving a surplus as of November 30, 1938, amounting to $14,762.00.

The A. Cressy Morrison Prizes of $200 each for the two most acceptable technical papers in a field of Science covered by the Academy or an Affiliated Society were awarded to the following papers entitled:

"The Morphology and Functional Evolution of the Atlas-Axis Complex from Fish to Mammals," by F. Gaynor Evans, University of New Hampshire.

"Oxygen Regulates the Dormancy of the Potato," by Norwood C. Thornton, Boyce Thompson Institute for Plant Research.

The A. Cressy Morrison Prize of $500 for the best paper on the origin of solar and stellar energy was awarded to the paper entitled:

"Energy Production in Stars," by Hans A. Bethe, Cornell University.

The following members were elected to Fellowship:

Anne Anastasi, Ph.D.
R. Earl Bowen, A.M., Ph.D.
Charles M. Breder, Jr., D.Sc.
Clarence C. Clark, M.S., Ph.D.
Charles Montague Cooke, Jr., Ph.D.
Frank Co Tui, M.D.
Lawrence K. Frank, A.B.
William Bayard Heroy, Ph.B.
Joseph C. Hinsey, M.S., Ph.D.
James W. Jobling, M.D.
Florence de L. Lowther, A.M., Ph.D.
Duncan MacInnes, M.S., Ph.D.
Alfred Plaut, M.D.
Raymond W. Root, A.M., Ph.D.

S. James Shand, Ph.D., Sc.D.
Luther C. Snider, A.M., Ph.D.
Harold E. Vokes, Ph.D.
Caroline Zachry, A.M., Ph.D.

Honorary Membership was conferred upon three eminent scientists, namely:

Edwin Grant Conklin, A.M., Ph.D., Sc.D., LL.D.
Ross Granville Harrison, A.M., Ph.D., M.D., Sc.D.
Irving Langmuir, Sc.D., Ph.D., LL.D., D.E.

The following officers were elected:—

President
A. Cressy Morrison

Vice-Presidents
Ida H. Ogilvie
Horace E. Wood, 2nd
Robert T. Rock, Jr.
Harry L. Shapiro
Duncan A. MacInnes

Recording Secretary
Frederick H. Pough

Corresponding Secretary
Roy Waldo Miner

Treasurer
Wyllys Rosseter Betts, Jr.

Librarian
John Hendley Barnhart

Editor
Erich M. Schlaikjer

Councilors (1938–1940)
W. Reid Blair
G. Kingsley Noble

Finance Committee
Herbert F. Schwarz
John D. Sherman, Jr.
Wayne M. Faunce

After the Business Meeting, the following program was presented:

"The Velocity of Chemical Reactions"
by
Victor K. LaMer

"On the Bottom of a South Sea Pearl Lagoon"
by
Roy Waldo Miner

The abstracts of these papers are included in this issue of the TRANSACTIONS.

NEW YORK ACADEMY OF SCIENCES

Address given at Annual Dinner and Meeting

DECEMBER 14, 1938

PROFESSOR VICTOR K. LAMER, Columbia University: *The Velocity of Chemical Reactions.* (This lecture was illustrated by lantern slides.)

In the development of the Science of Chemistry, chemists first concerned themselves with the identification and discussion of the various specimens of matter. These inquiries led to the discovery and classification of the chemical elements. The use of the balance yielded data which led to the quantitative laws of chemical combination which later resulted in the introduction of chemical formulae as a shorthand method of designating and describing the properties of matter. The molecular architecture implied in these structural formulae hastened the development of synthetic chemistry. We are indebted to the synthetic practical chemist for producing at low cost, from the raw materials, coal, tar, coke, and petroleum, a myriad of valuable compounds which find abundant use as medicinals, dyes, perfumes, photographic materials, artificial resins and solvents. Although these synthetic products often exist as natural products, many of them have no counterpart in Nature and therefore represent a truly creative development.

The problems of identification, description, purification, and synthesis, have, until the last few decades, been the principal occupation of chemists. Lately, there has been an increasing interest in extending the theoretical aspects of the Science to answer the questions of how and why chemical reaction proceeded. It is this phase of the subject which is the special province of the physical chemist, of whose work I wish to speak tonight.

The greatest single triumph of physical chemistry has been the development of methods for calculating and predicting the

equilibrium state of chemical process. By this term I imply the prediction of the yield in a chemical reaction. These predictions are made by investigating the energy relations for the reaction. This field of inquiry is designated by the erudite term, thermodynamics, and involves the laws governing the conversion of heat into work and chemical equilibrium.

All of the important principles of thermodynamics are embodied in three laws which have been fully established within the last decade. There remains only the problem of working out the details of the applications to special cases. Knowing the heat exchange involved in the reaction, thermodynamics teaches us to calculate precisely the change in yield from the reaction when the temperature and pressure are varied. In order to calculate an absolute value of the yield, thermodynamics requires one value for the yield at some one temperature and pressure. Even this restriction can be removed today by the aid of the third law, if the heat capacities of all the components have been measured to sufficiently low temperatures.

Within the last decade, another special field known as statistical mechanics has developed so that it is now possible to calculate the equilibrium state of a reaction at any temperature and pressure without requiring any measurement to be made upon the reaction in question. Such an "a priori" calculation is indeed an important advance. It is only necessary to know the energy levels of the molecules involved. This information on energy levels is obtained by the spectroscopist from the band spectra of the component substances, and while the exact calculations are still restricted to the gaseous state, new data and methods are accumulating so rapidly that it is possible to make a fairly satisfactory "a priori" prediction in the liquid state. It is by means of such calculations, that my colleague, Professor Urey, first became convinced that it should be possible to isolate the isotopes of hydrogen, nitrogen and carbon. These preliminary calculations have been of inestimable value in guiding the experimental researches to success.

The predictions of thermodynamics unfortunately neglect one important feature, namely, the time factor. Thermodynamics is not competent to state how long a time will be consumed in reaching the equilibrium state. It does the chemical manufacturer very little good to tell him that the process he proposes to use will give him a 99 per cent yield, but that, unfortunately, it will probably take 1,000 years to achieve that high yield.

Chemical kinetics, on the other hand, deals specifically with the problem of hastening the velocity of chemical reactions. It is therefore of the utmost importance to the theoretical and practical chemist.

Thermodynamics has been called legislative in character, while kinetics is executive in character. To borrow from the language of the stock broker, thermodynamics tells exactly what will happen when, as, and if issued, but says nothing at all about how long it will take before the stock will be issued. Perhaps the SEC will not permit the stock to be issued. Observe if by executive action, one can speed up the rate of a chemical reaction, the interest charges upon the investment for equipment in a plant are increasingly reduced.

What do we know about chemical kinetics? Well, compared with thermodynamics, it is still darkest Africa.

If $A + B \rightarrow P$ represents the kinetic reaction between the molecules A and B to form the product P, then we know that if we double the concentration of A; *i.e.*, double the number of molecules of A per unit of volume, the rate is doubled because the number of collisions is doubled, and similarly for the B molecules. Hence doubling the concentration of A and B produces a fourfold increase in rate.

The chemist can therefore modify the rate within limits by controlling the concentration. But what about the absolute rate at unit concentration? Why are some reactions rapid and others slow? In the first place, not every collision between A and B results in reaction. If it did, every gas reaction would proceed with explosive violence since the number of collisions

per second is a prodigiously large number. Only the very exceptional molecules, designated henceforth by the symbol "X" will react. The special characteristic of these X or activated molecules is that the molecules in this critical state of reactivity possess more energy than do those in the normal or nonreactive state.

If every binary collision were fruitful, the rate of reaction would increase only about 4 per cent for every ten degree rise in temperature for that is the rate of increase in number of collisions with the temperature. As a matter of fact, van't Hoff found that the rate of most reactions increases about 300 to 400 per cent over a ten degree interval but in an exponential manner.

The molecules in a gas move with different velocities. The majority move at a velocity very nearly equal to the statistical average velocity. The temperature of the gas is simply another way of expressing this average velocity. Only a very few molecules move with a velocity much greater than the average. When two molecules of average velocity collide, they bounce apart and nothing further happens. When two very rapidly moving molecules collide, the extra energy of translation disrupts them. They exchange partners and, instead of dancing, a reaction occurs. The energy of activation arises then from the translational energy involved in the collisions of very fast or hot molecules.

According to Maxwell and Boltzmann, the chance that a molecule will have an exceptionally high velocity is exponentially related to its translatory energy E. The law for the temperature dependence of the rate of a reaction, consequently, has the same form. In fact we find that

$$\text{rate} = Ze^{-E_{act}/RT}$$

is a formula which often works remarkably well in gases in spite of its mathematical simplicity. Z is the number of collisions.

Today physical chemists are representing the energy relations between different molecules and their reaction products

by energy diagrams which resemble the contour maps of a mountainous country. The number of interchanges in the population of people living in the valleys separated by a mountain pass is determined by the number of people in the community, the height and character of the roads leading over the mountain pass, and the mountain climbing ability of the natives. The modern concept of reaction kinetics is well portrayed by this geographical and ethnological illustration.

The higher the mountain pass, which is the same thing as saying that the molecules require a large energy of activation to react, the fewer the interchanges. The more roads that are constructed over the mountain pass, the more easily it can be traversed. Until recently, theory has neglected the possibility of more than one road over the mountain pass. The entropy of activation is the term which expresses this factor. Its importance is being recognized as result of intensive work by the author at Columbia University and by Professor Eyring at Princeton University.

It does not require any great stretch of the imagination to recognize that a sharpshooter, with a machine gun, stationed at the top of the mountain pass, can effectively prevent many climbers from traversing the pass. This concept underlies the secret of the explanation for negative catalysis. While it is very important to be able to speed up chemical reactions in order to reduce the capital outlay for plant equipment, it is equally important to inhibit the deteriorating chemical processes which destroy valuable products. One of the most fascinating recent developments has been that of negative catalysis.

My own researches at the present time deal with the problem of determining the effect of electric charges upon the molecules through their effect upon the energy of activation and the entropy of activation.

If we know the rate at which $A^0 + B^0 \rightarrow P$, how rapidly will $A^+ + B^-$ or $A^- + B^-$ react? It requires only an elementary knowledge of electrostatics to recognize that the reaction between ions of the same sign will be slower because the electrical re-

pulsive forces will prevent collisions. This is found to be the case; on the other hand, if we make the molecules of opposite electric charge, a very marked increase in rate occurs because of the greater number of collisions. A^+ and B^- will, on the average, be closer together than will A^0 and B^0. Quantitatively we find the influence on the rate is in exact accord with electrostatics. It depends upon the square of the electric charge, and inversely upon the square of the radii of the molecules and inversely as the dielectric constant, just as Coulomb's law predicts.

If we change the solvent we change the rate of reaction, and in the case of ionic reactions by an enormous amount. The action is primarily through the dielectric constant. If we pass from water to alcohol, a reaction of the type $A^+ + B^-$ is speeded up because the electric fields are greater in the lower dielectric medium, alcohol, than in the higher dielectric medium water. On the other hand, a reaction of the type $A^- + B^-$ or $A^+ + B^+$ is slowed down on adding alcohol as would be expected.

It has been known for a long time that the addition of salt markedly reduces or increases the rates of ionic reactions. If the reaction is of the type $A^+ + B^+$, salt additions increase the rate. If of the type $A^+ + B^-$, salt additions decrease the rate. This is due to the fact that the numbers of collisions are affected by the electric charges of all the ions in the solution. This last effect is known as Primary Salt Catalysis. For many years Salt Catalysis remained an enigma of physical chemistry, but today we can confidently predict its magnitude. These researches represent one of the first definite examples of how a Catalyst works.

NEW YORK ACADEMY OF SCIENCES

Address given at Annual Dinner and Meeting

DECEMBER 14, 1938

DOCTOR ROY WALDO MINER, American Museum of Natural History: *On the Bottom of a South Sea Pearl Lagoon.* (This lecture was illustrated by colored lantern slides and motion pictures.)

The Island of Tongareva (also known as Penrhyn Island) is a coral atoll situated in the South Pacific 9° S. and 158° 2′ W. It is 1847 miles south of Honolulu, and almost on the same meridian, the longitude of that port being only 30′ farther west. The islets of which it is composed, called *motus* by the natives, surround a central lagoon about eleven miles in diameter. They are connected by coral reefs awash at low tide, except at three entrances, one at the north-east; one at the north-west; and a third on the western border of the atoll. Only the latter can be navigated by vessels of larger size.

The island is low-lying, being nowhere more than 50 feet above sea-level, and is covered with luxuriant growths of coconut trees. It is inhabited by about 500 indigenous Polynesians gathered in two settlements, Omoko and Tetautua, on the east and west shores of the atoll respectively. The lagoon varies in depth, from 15 to more than 200 feet. It is floored with sand and mud but many coral shoals rise from the bottom, often reaching within a foot or so of the surface. Many others occur in process of upward growth. The shoals are particularly numerous in the western part of the lagoon. Pearl-shell occurs abundantly over about a quarter of the lagoon bottom, and the waters are alive with gaily colored fishes.

The natives live on various dishes made from coconut and on the fish and edible mollusks captured in and about the atoll. The chief exported products are copra and pearl-shell. Trading schooners enter the lagoon and tie up at the wharf of

Omoka about twice a month to load with copra and pearl shell, as well as palm fibre hats, mats, and various other articles woven by the native women.

On September 18, 1936, an expedition from the American Museum of Natural History arrived off the West Passage of Tongareva led by the writer in cooperation with Mr. Templeton Crocker of San Francisco, who accompanied the expedition to put the services of his yacht *Zaca* and its crew of 17 men at the disposal of the party. Also from the Museum were Wyllys R. Betts, field associate; Chris Olsen, artist and modeler; and Wiliam F. Coultas, ornithologist. The latter was on his way to Samoa and other Pacific Islands to secure birds for museum groups. Mr. Betts, Mr. Olsen, and I had come to Tongareva to study the pearl oyster beds of the island and the methods by which the natives secured the pearl shell. Thus we could obtain material and accurate data from which a Pearl Fisheries Group could be constructed for the Hall of Ocean Life in the American Museum. In this work, we also received invaluable aid from Mr. Crocker's staff, particularly, Toshio Asaeda, Japanese artist and photographer, and Frank Tiaga, a native Samoan sailor. Mr. Philip Woonton, British Government Agent at Tongareva, gave us special facilities and put at our disposal Tau, a native diver, to act as guide and pilot.

We were fortunate in having two weeks of continuously favorable weather for our purposes. Our vessel had been guided into the lagoon through the narrow and hazardous West Passage by native pilots and was brought to anchor off the village of Omoko, the picturesque huts of which snuggled under the towering groves of coconut palms. The first three days were spent exploring the lagoon with a launch under the guidance of Mr. Woonton and Tau. After we had selected the locations best adapted for our work, we loaded our cameras, undersea boxes, and film in an otter boat equipped with a motor, while the diving outfit consisting of two helmets, two pumps, undersea tripods, brass-rope ladder, and hose were packed into a dory, which was taken in tow. The dory also

carried water glasses, nets, and various fishing appliances. Two sailors from the *Zaca* manned the otter-boat together with Tau and Toshio, the Japanese artist. Betts, Olsen, and I, with Frank, the Samoan, occupied the dory. When we arrived alongside one of the table-like shoals, the dory was secured fore and aft by carrying two anchors out and hooking them on to the top of the spreading corals. The otter boat was lashed parallel to the dory on the outer side, and the brass-rope ladder was lowered from the inner side. Though the shoal-top lay close to the surface, its sides descended precipitously to a depth of twenty-five feet or more. After examining the bottom with a water-glass, I stood on the rope ladder with the water washing my shoulders while the diving-helmet was lowered over my head and a pump was started going. The rungs of the ladder, constructed of brass piping, were placed a foot apart, so that, by counting them as I descended, I could measure my depth as I stepped off at the bottom. I could then see the cliff-like sides of the coral shoal rising above me, luxuriantly overgrown with corals of many species, ranging through every imaginable tint of color, while in form they varied from massive dome-like growths to branching clusters of the utmost delicacy and lace-like fragility. In one particular gorge, afterward chosen for the scene to be reproduced in the proposed Museum group, I recognised various species of *Siderastraea, Porites, Galaxea, Pocillopora, Turbinaria, Fungia, Leptoseris, Podabacia, Merulina* and many others, while the genus *Acropora* was present in overwhelming abundance, ranging from arbusculate to paniculate growths.

Along the sea-floor here and elsewhere occurred patches of pearl-oysters (*Avicula margaritifera*) from a few individuals to numerous clusters. For the most part, they dotted the sea-bottom here and there, either lying flat on the sand, or growing in clusters of two or three on the dead coral rock. The valves were about eight inches in diameter. When alive, the edges of the shells were usually slightly parted, with flat finger-like projections extending from the margin representing the

radiating lines of growth between which the shelly material filled in as it grew outward. The outside of the shell of the living oyster was usually overgrown with calcareous algæ, bryozoa, and encrusting sponges, of various contrasting colors.

A smaller species of pearl shell, called by the natives the *pipi* shell, also occurs. Associated with the corals and pearl shell in the neighborhood of the shoals is the smaller *Tridacna* (*T. compressa*). This occurs partially embedded in the coral rock or in the sand, and sometimes growing loosely on coral clumps. It is about a foot in length and is a miniature of the great *Tridacna gigas,* its near relative, characteristic of the Philippines, the Carolines, and the Great Barrier Reef. There is no space here to mention the many species of echinoderms, annulates, crabs, shrimp, and other forms of life comprising the remainder of the reef association, but hosts of these were observed, recorded, and collected. The reef fishes were particularly abundant and varied. Their vivid colors and often grotesque forms were conspicuous as we stood on the sea-floor.

We had two diving helmets and pumps, so that either Betts or Olsen were working undersea with me most of the time. Our tripods were lowered to us on a cord and placed in position. Then the cameras encased in specially contrived undersea boxes were lowered, and photographic records by motion pictures and stills were made in waters so transparent that there was no difficulty in photographing 25 feet below the surface.

Tau, the Tongarevan diver, and his companion, Tony, would dive down and wrest clusters of shell from the sea-bottom, while we photographed them in the act. Chris Olsen carried palette and oil colors undersea and made oil sketches from life on oiled canvas stretched over plate glass. Many specimens were brought up alive and placed in aquaria on the *Zaca* that detailed color studies might be made of them. We caught the brilliant reef fishes by hook and line and in fish-traps. Others that could not be secured in any other way were

stunned by using the "bang-bang," a bamboo pole, to the end of which dynamite caps were attached, connected by an insulated electric wire with a switch box in the dory. We would stalk fishes over the sea bottom, using diving helmets, and holding the "bang-bang" out like a fish-pole until the caps were over the fish in question, when we would jerk the cord as a signal, the sailor in the dory would close the switch, and the caps would explode, stupefying the fish. Tau would then dive down with a hand-net, scoop up the fish, swim to the surface and turn over his catch to Toshio, who would place it in a pail of sea water. After a time the fish would recover and Toshio would make accurate sketches of it in color. The fishes thus captured were brought back to the *Zaca* and plaster molds were made of them, while the original specimens were preserved in fluid.

By these various methods, we not only made first hand observations and records of the reef association in which the pearl oyster occurs, but secured an abundance of specimens. The collections included more than 10 tons of coral, hundreds of pearl shell, and large series of other invertebrates.

The Pearl Divers Group is now being constructed at the American Museum and, when completed, will be a trustworthy record of the ecological conditions under which the pearl shell is found in a typical island of the South Pacific.

SECTION OF ANTHROPOLOGY

DECEMBER 28, 1938

DOCTOR GEORGE C. VAILLANT, American Museum of Natural History: *The Documentation of the Archaeology of the Valley of Mexico.* (This address was illustrated by lantern slides.)

The techniques for amassing information have so governed American archaeology in the last twenty years, that the vast bulk of data on time and tribe acquired in that period is seldom intelligible to the non-professional reader. For most of the New World there are no native historical records so that knowledge of the Indians' past must largely depend on such material culture remains which have resisted the destructive action of time and weather. Therefore, any place or culture which has a documentation is extremely important in American archaeology and the Valley of Mexico is preeminent in this respect in that it is the source of more historical manuscripts than any of the past of Indian America. Furthermore, the archaeological sequences in this region are very well known.

There are four main stages to the development of human history in Central Mexico. The latest is the Aztec period, which is preceded by a phase of immigration involving various types and stages of culture. This we call the Chichimec period and it supersedes a third period of unified culture, known as the Teotihuacan or Toltec. The fourth and earliest epoch in the Valley reveals people with a fair technical development, but without the ceremonial expression found in the later periods. This Early Culture of Archaic phase is by no means primitive and archaeology has not yet revealed these steps found elsewhere in the Americas whereby man levered himself from a hunting and food gathering role to that of a sedentary farmer.

Most of our records come from the Aztec period and are either picture writings or else commentaries based on similar

documents which have been lost. Most of the manuscripts refer to the histories of individual city states, tribute rolls, genealogies, maps, etc., which produce much additional information. A curious feature of this documentary evidence is that much of it was composed in the half century following the Spanish Conquest and many older pictographic records seem to have been copied at this time. Therefore the net effect is to have in addition to an analysis of Indian civilization in Mexico prior to the Conquest, an interesting picture of the persistence of Indian culture in Early Spanish Colonial times.

The city states of the Valley of Mexico were usually founded by groups of immigrants during the Chichimec period. In a number of cases notably Tenochtitlan (the ancient Mexico City) and Texcoco, the local annals describe and picture this period of migration and early settlement. Other records compiled from picture records, but set forth in European script, utilizing in some cases the Spanish language and others Nahuatl, provide further information on Chichimec times. In this fashion some four centuries of history can be reviewed, supplementing and being supplemented by the archaeological record.

The Teotihuacan period, historically speaking, is poorly covered. Our source material is traditional and was passed on by scribes and annalists in Chichimec and Aztec times. Yet even here the archaeological record is implemented by these ancient memories and in turn the techniques of excavation have provided data which render authentic these ancient tales.

As a result vividly and accurately one can discern the evolution of culture, and how various culture traits were transmitted. Trade and the payment of tribute undoubtedly influenced material culture, but in addition there is mention of groups of people breaking away from a parent community to join a new one, a condition sometimes arising from overpopulation and sometimes from political dissensions within the community. Tributary groups in some cases absorbed various traits from their conquerors, and the reverse condition is

also true. The idea of emulation seemed also strongly to operate, and there are several examples of the spread of the idea of chiefly lineage, which resulted in the gradual disappearance of the earlier and more primitive group council rule. The state of ferment in Central Mexico in the centuries preceding the Conquest is clearly brought out in the chronicles, and the parallel to European conditions is very apparent, a cultural unity spreading and developing in the face of the most violent political dissension.

There is a rich field awaiting a historically inclined ethnologist in the Central Mexican field, where so much data, unrecoverable by direct archaeological techniques, are here preserved. The study of acculturation problems could also profitably be extended to the post-Conquest era, and the student of modern Mexican ethnology would find much to guide him under these almost laboratory conditions. Much of the material exists in print, thanks to the labors of the nineteenth century scholars, but the great advances in sociology and ethnology warrant a general re-examination of the published data.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 FEBRUARY, 1939 No. 4

SECTION OF BIOLOGY[1]

JANUARY 9, 1939

DOCTOR H. HERBERT JOHNSON, College of the City of New York: *Twins, Normal and Abnormal.* (This lecture was illustrated by lantern slides, clay models, and specimens.)

In spite of the familiarity of the topic, it is obvious that much is still to be learned concerning the exact origin in embryo of human twins and partial twins, while at the same time there exist widespread misconceptions regarding the phenomenon of twinning. A review of current ideas bearing upon the problem seems timely, especially since opportunity is given to illustrate some points with preserved specimens of unusual interest. No attempt is made here to present very original points of view on the matter, and the discussion is limited to well known theories of special interest to the embryologist.

Dizygotic ("fraternal" or two-egg) human twins theoretically may result from any one of three possibilities: 1. More or less simultaneous ovulation from both ovaries; 2. Ovulation, at about the same time, of more than one follicle in a single ovary; 3. Ovulation of a single follicle containing two or more ova. Evidence is presented that such biovate or polyovate follicles are much more frequent than is generally believed. The exhib-

[1] No meetings were held in January by the Sections of Geology and Anthropology.

TRANSACTIONS of the New York Academy of Sciences, Series II, Volume 1, No. 4, February, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

its include examples from the pig, rabbit, dog, cat and monkey. The view of Maximov that such follicles are characteristic of young females, is not supported by this investigation.

It is now quite generally accepted that about one fourth of all twins are monozygotic ("identical" or single-egg) but no single line of evidence gives irrefutable proof that monozygotic twins exist. The various sources of evidence which collectively tend to establish proof of single-egg twins are reviewed in the recent book by Newman *et al.*[1]

Statistical evidence is gleaned from the fact that same-sexed twins occur more frequently than opposite-sexed twins, whereas the genetic expectation is a 1 : 1 ratio of same-sexed to opposite-sexed pairs. The weakness in this line of evidence when considered alone lies in the assumption that opposite-sexed twins are equally viable in utero as same-sexed pairs, and are in no way subject to environmental sex changing disturbances such as have been demonstrated experimentally in other higher vertebrates.

Very important is the fact that conjoined twins are always of the same sex, and so are twins born in a single chorion. Until 1928 it was generally believed that *all* monozygotic twins develop in a common chorionic envelope. In 1927 Komai stated: "Apart from the examination of the placenta and foetal membranes at birth, there is *no* safe criterion of distinguishing the identical twins from the fraternal twins."[2]

The fact that identical twins are so frequently monochorial refutes a widespread dogma that such individuals originate by a splitting apart of the blastomeres at first cleavage. This point of view results from a misguided application to the human egg of the classical *experimental* results obtained on other forms. Chabry, Herbst, Driesch, Wilson *et al.* obtained more or less "normal" development of certain marine invertebrates and of Amphioxus from single blastomeres isolated experimentally during early cleavage. It should be emphasized that

[1] Newman, Freeman, and Holzinger. "Twins: A Study of Heredity and Environment." University of Chicago Press, 1937.

[2] Komai, Taku. Science, 65: 280. 1927.

these eggs do not self-isolate their own blastomeres. There may be no theoretical reason why the human ovum, or that of any mammal, might not divide at a stage in early cleavage into entirely separated blastomeres each capable of producing a complete body. The presence of the single chorionic envelope, however, shows that this is not usually the case. Since the chorion in the mammal embryo is developed from the outer layer of cells of the blastula and the embryo results from a clump of more centrally placed cells, it is obvious that the real division of the body in such cases results from the separation of this inner cell mass at a comparatively advanced stage of blastula formation. Such duplicated inner cell masses have been described in the armadillo by Patterson, the sheep by Assheton, and the human by Arey.[3,4]

Since 1928 some evidence has accumulated to show that perhaps a few monozygotic twins are indeed dichorial. The case of identical triplets described by Komai and Fukuoko,[5] 1931, is not altogether conclusive when considered alone. Two bodies were produced in a chorion, and the third was in a separate chorion attached to the first. Komai interpreted this as a fusion of two separate chorions, but it could almost equally be called a faulty separation of one. Other evidence for dichorial identical twinning is presented by Curtius[6] and Lassen.[7] It is still unknown whether these cases really represent a separation of early blastomeres, or a constriction of the chorion at a much later stage, perhaps while invasion of the uterine wall is taking place.

Duplication of body parts occurs in an extensive series ranging from duplication of a single digit or the palm of the hand to duplication of the entire limb, the head, or practically the entire body (the conjoined, or "Siamese" twin). Conjoined

[3] Arey, L. B. Anat. Rec., 23, 1922.

[4] Arey, L. B. "Developmental Anatomy," 3rd Ed., p. 154, 1934. W. B. Saunders, Publisher.

[5] Komai, Taku and Goro Fukuoka. Jour. of Hered., 22, 1931.

[6] Curtius, F. Zeitschrift für Konstitutionslehre, 13, 1928.

[7] M. T. Lassen. Archiv für Gynakologie, 147, 1931.

twins apparently arise later in gastrulation than identical twins. The dissimilarity of the individuals has been explained in a view advanced by Newman.[8] One is developed entirely from that part of the embryonic disc previously differentiated as left, and the other from the corresponding rudiment already determined as right. Each rudiment restores the missing portion in mirror-image symmetry to itself. Thus the two bodies differ to about the same degree as is frequently seen in the left and right sides of an ordinary person.

After reviewing various theories as to the basic causative factor leading to embryonic axial duplication, the theory of developmental arrest, advanced especially by Stockard,[9] still seems to be, in a general way, the best interpretation. The Spemann school has shown that the portion of the embryonic disc capable of being organized to produce a head is a considerably larger field than is actually necessary to produce a head. The center is the more responsive portion of this field. If a developmental arrest occurs precisely at the time that evocatory stimulation, capable of organizing a head, is expected, the center of the field is retarded and loses its power of development. If the retarding factor then ceases to operate, some power of organization still is retained by the more lateral portions of the head-field, and these separately begin development, thus forming a duplicated head. Similar arrest, occurring at the critical moment at which gastrulation should begin, might similarly result in the appearance of two gastrulation points on a single embryonic disc. The degree of mechanical and physiological separation of these centers is in direct ratio to the degree of separation exhibited by the resulting twins.

Specimens exhibited included a foetal pig with duplicated snout and jaws, and three eyes; a human foetus of five months with two complete heads, four shoulders, and three arms, a human octopus, with two nearly complete bodies joined at the pelvis These specimens, and others shown, are part of the collection of the Division of Embryology of the College of the City of New York

[8] Newman, H. H. Journ. of Hered., 22, 1931.

[9] Stockard, C. R. Amer. Jour. Anat., 28, 1921.

SECTION OF PSYCHOLOGY

January 16, 1939

Professor John A. McGeogh, Wesleyan University: *Conditions of Human Forgetting.*

The decrements in learned performance which are correlated with the passage of time without practice were one of the first phenomena of human learning and memory to be studied experimentally (Ebbinghaus). There has been until recently small interest in the conditions which produce these decrements, that is, in forgetting. This is the more strange in view of the way in which forgetting pervades both formal learning and everyday experience. It is present in all formal learning and among other things is basic to the form of learning curves. In daily life it reaches from the forgotten errand to the largely forgotten body of knowledge.

Within the last few years attempts to account for forgetting have been made within the level of the phenomenon to be explained. Two major conditions have been discovered experimentally: interference from interpolated activity (retroactive inhibition) and changed stimulating conditions.

Retroactive inhibition, first described by Müller and Pilzecker at the turn of the century, has been the more studied of the two. It refers to the disintegrating influence exerted by a subsequent activity upon the retention of something which has been learned antecedently. A better definition is the operational one provided in the experimental paradigm:

Control condition:	Learn (1)	Rest	Measure retention of (1)
Experimental condition:	Learn (2)	Learn (3)	Measure retention of (2)

The interval between learning and the measurement of retention is the same under the two conditions and other factors are kept as constant as possible. The difference lies in the interpolated learning (3) under the experimental condition. If retention under this condition is less than it is under the con-

trol, this greater decrement is the phenomenon called retroactive inhibition.

It has been found in the laboratory with a wide range of materials and of learners. It appears with all groups of subjects who have thus far been studied, with both meaningless and meaningful materials, and by all methods of measuring retention. Its amount may vary from a percent or two to 90 percent or higher. The amount in which it appears is a function of a number of conditions.

One of the most general of these conditions is the relation (usually called similarity) between the material first learned (original material) and the interpolated material. The similarity between these two may be of several kinds or dimensions. (1) Especially when the materials are serial lists, they may vary in degree of identity of formal construction, as when the two halves of a memory span have varying percentages of common terms or two lists of syllables contain terms with common letters. (2) The two materials may vary in similarity of meaning, as when the original list is composed of adjectives, the interpolated list of their synonyms or their antonyms, of unrelated adjectives, nonsense syllables or three-place numbers. (3) The two may be similar, not in themselves, but in the ways in which they are presented or dealt with by the subject.

Retroaction varies with percentage of identity, decreasing in the memory span case as the percentage decreases. Similarity is difficult to quantify, but it can at least be ranked by competent judges. In a considerable number of experiments it has been found that as similarity decreases from near identity amount of retroactive inhibition decreases, until it approaches zero as the similarities between the two materials become slight.

Other conditions of which it is a function are degree of learning of both original and interpolated materials, the instruction to and set of the subject, and the amount of material to be learned.

It is certain that interpolated activity produces a decrement under the conditions of the laboratory. Are formally learned

materials susceptible to interference from the activities of daily life? This has been answered in the affirmative by several different experiments, the first of which was by Jenkins and Dallenbach. They found that lists of nonsense syllables were forgotten according to the classical Ebbinghaus curve when the subjects went about their ordinary daily activities during from one to eight hours following learning. When sleep filled the interval, however, the retention curve dropped during the first two hours, though less rapidly than when waking activity intervened, but did not fall thereafter.

Two theories have been offered to account for the interference produced by interpolated activity. The perseveration theory holds that the neural activities involved in learning perseverate (continue) for some time after practice ceases, thus giving a kind of gratuitous practice. Interpolated activity damps this perseveration. This theory lacks sufficient neurological basis and is, moreover, unable to incorporate the behavioral facts. The transfer theory, on the other hand, holds that the interference is a matter of confusion between the materials or methods of the original and interpolated activities, and is a form of negative transfer. The facts of the experiments on similarity support this strongly, as does the discovery that specific parts of each one of the two activities actually carry over and become incorporated into the other.

A second condition of forgetting, though it has been less explored by experiment, is an altered stimulating condition. Everything learned is learned in response to stimuli, and measured retention is in response to a part of the original stimulus situation. If the stimulating conditions at the time of attempted recall are sufficiently different from those during learning, recall will fail.

The experiments by Pan, Dulsky and others have begun an exploration of the changes in stimulating context which will produce forgetting. It seems that not all changes will yield a decrement; some certainly will. The operation of this condition in daily life is seen in the failure to recall the name of an

acquaintance when met in a new environment, or the inability to recall a recently known fact when not in the classroom or study where it was learned.

Other conditions, not yet experimentally studied, may operate to produce forgetting. Enough is known of the influence of these two, however, to make one wonder that we remember anything, rather than why we forget as much as we do. In actual life each new experience serves as interpolation after the experience that went before, and after each experience interpolations progressively accumulate. We seldom try to recall in the same context of stimuli as that in which the material was learned. That we retain as much as we do is a function of a favorable combination of the conditions which determine amount of retroactive inhibition and of forgetting from altered stimulation. Research upon these conditions has only recently been begun and offers a wealth of experimental problems.

ANNOUNCEMENT OF PUBLICATIONS FOR 1939

The following publications will be issued by the Academy during the current year. Members of the Academy who desire to receive these papers will kindly request the Executive Secretary to send them, and they will be mailed, free of charge, as they are ready for distribution except as qualified in the footnotes:[1]

SPECIAL PUBLICATIONS:

1. VOLUME I. CLIMATE AND EVOLUTION. By William Diller Matthew. Enlarged and revised to include the following papers:
   - "Climate and Evolution," by William Diller Matthew.
   - "Some Remarks upon Matthew's 'Climate and Evolution,'" by T. Barbour; with Supplementary Note by William Diller Matthew.
   - "Note on the Wegener Hypothesis, Supplementary to 'Climate and Evolution,'" by William Diller Matthew. (Previously unpublished.)
   - "Affinities and Origin of the Antillean Mammals," by William Diller Matthew.
   - "The Dispersal of Land Mammals," by William Diller Matthew.
   - "The Outline and General Principles of the History of Life," Synopsis of lectures in Palæontology I, University of California Syllabus, Series 213, Figs. 8–12 (Tables Showing the Past and Present Zoogeographical Distribution of Land Mammals), by William Diller Matthew.
   - "Annotated Bibliography of William Diller Matthew," by Charles Lewis Camp and Vertress Lawrence VanderHoof.

[1] Student and Associate Members are entitled to receive one complete monograph or up to 150 pages.

ANNALS:

2. "Constitution and By-Laws, including a full membership list." This will be sent to all members.
3. "Electrophoresis." This article will contain the five papers delivered at the conference of the Section of Physics and Chemistry, October 28 and 29, 1938. (About 50 pages.)
4. "American Cities and States: Variation and Correlation in Institutions, Activities, and the Personal Qualities of the Residents," by Edward L. Thorndike. (About 104 pages.)
5. "The Morphology and Functional Evolution of the Atlas-Axis Complex from Fish to Mammals," by F. Gaynor Evans. (About 72 pages.)

SCIENTIFIC SURVEY OF PORTO RICO AND THE VIRGIN ISLANDS:[2]

Volume VIII, Part 3: "Diatomaceae," by Robert Hagelstein. Published January 19, 1939.

Volume XVI, Part 2: "Annulata of Porto Rico and the Virgin Islands," by A. L. Treadwell.

Volume XIV, Part 3: "Insects of Porto Rico and the Virgin Islands—Heteroptera," by H. G. Barber.

[2] The Scientific Survey publications are available to Members for $1.50 per part and to non-members for $2.00 per part.

A certain number of parts, as announced each year, are available free to Honorary, Life and Sustaining Members.

NEW MEMBERS

It will be of interest to the members of the Academy for us to publish herewith the list of new members added to the rolls of the Academy during the year from February 7, 1938, to February 6, 1939.

HONORARY MEMBERS

Conklin, Edwin Grant, Ph.D., Sc.D., Professor Emeritus of Zoology, Princeton University, Princeton, N. J.

Harrison, Ross Granville, Ph.D., M.D., Sterling Professor of Biology, Yale University, New Haven, Conn.

Langmuir, Irving, Ph.D., Sc.D., Associate Director, Chemical Research Laboratory, General Electric Company, Schenectady, N. Y.

LIFE MEMBER

Cooke, Charles Montague, Jr., Ph.D., Bishop Museum, Honolulu, Hawaii.

SUSTAINING MEMBERS

Akin, Charles V., M.D., U. S. Public Health Service, U. S. Quarantine Station, Staten Island, N. Y.

Armstrong, John Charles, M.A., Harvard University, Cambridge, Mass.

Boldt, Hermann Johannes, M.D., White Plains, N. Y.

Carabba, Victor, M.D., New York, N. Y.

Gregory, Angela DuBois, R.N., New York, N. Y.

Levy, Milton, Ph.D., Assistant Professor, Biochemistry, New York University College of Medicine, New York, N. Y.

MacInnes, Duncan A., Ph.D., Physical Chemistry, Rockefeller Institute, New York, N. Y.

Martin, Esmond B., Mineralogist, New York, N. Y.

Pough, Francis H., Chemist, St. Louis, Mo.

Pyle, Norman J., M.D., Lederle Laboratories, Pearl River, N. Y.

Rivers, Thomas Milton, M.D., Sc.D., Director, Hospital, Rockefeller Institute, New York, N. Y.

Robbins, William J., Ph.D., Sc.D., Director, New York Botanical Garden, New York, N. Y.

Rosett, Joshua, M.D., Professor, Neurology, Columbia University, New York, N. Y.

Russell, Charles, Ph.D., Curator, Education, American Museum of Natural History, New York, N. Y.

St. John, Fordyce B., M.D., Sc.D., Assistant Professor, Surgery, Columbia University, New York, N. Y.

Schaefer, Hugo H., Phar.D., Ph.D., Dean, Brooklyn College of Pharmacy, Brooklyn, N. Y.

Taylor, Harden Franklin, Sc.D., President, Atlantic Coast Fisheries Co., New York, N. Y.

Weber, Orlando F., Chemist, New York, N. Y.

White, Amelia A., New York, N. Y.

ACTIVE MEMBERS

Abel, Theodora Mead, Ph.D., Director of Research, Trade Extension Classes, Board of Education, New York, N. Y.

Ackermann, Edward A., M.D., New York, N. Y.

Ackermann, Wolfgang, M.D., Instructor, Anatomy and Surgery, Columbia University Medical School, New York, N. Y.

Aldridge, Virginia Downward, B.S., Science Department, Newark Museum, Newark, N. J.

Applebaum, Edmund, D.D.S., Assistant Professor, Dental and Oral Surgery, Columbia University, New York, N. Y.

Andersen, Olaf, D.Sc., Consulting Petrologist, U. S. Steel Corporation, Kearney, N. J.

Babcock, Harriet S. B., Ph.D., Psychologist, Vocational Adjustment Bureau, New York, N. Y.

Bachman, George W., Ph.D., Director, Columbia School of Tropical Medicine, San Juan, Puerto Rico.

Bailey, Percy Lawrence, Ph.D., Assistant Professor of Biology, College of the City of New York, New York, N. Y.

Barry, Frederick, Ph.D., Associate Professor, History of Science, Columbia University, New York, N. Y.

Beach, Frank A., Ph.D., Assistant Curator, Experimental Biology, American Museum of Natural History, New York, N. Y.

Bergmann, Max, Ph.D., Rockefeller Institute, New York, N. Y.

Biddle, Russell Lee, Ph.D., Assistant Professor, Biology, College of the City of New York, N. Y.

Binkley, James Samuel, M.D., Memorial Hospital, New York, N. Y.

Booth, Verne H., Tutor, Geology, Brooklyn College, Brooklyn, N. Y.

Brackmier, Gladys, Geology, Columbia University, New York, N. Y.

Brand, Albert R., Cornell University, Ithaca, N. Y.

Brickner, Richard M., M.D., Assistant Professor, Neurology, Columbia University, New York, N. Y.

Brown, Frederick W., A.M., Speech Pathologist, Public School, Floral Park, N. Y.

Bucky, Philip Barnett, E.M., Associate Professor, School of Mines, Columbia University, New York, N. Y.

Bull, Titus, M.D., New York, N. Y.

Burley, Joseph Cilley, Physicist, Bendix Corporation, Bendix, N. J.

Cantala, Julius, M.D., New York, N. Y.

Carpenter, Clarence Ray, Ph.D., Anatomy, Columbia University, New York, N. Y.

Casperson, William C., Mineralogist, Paterson, N. J.

Cattell, McKeen, Ph.D., M.D., Associate Professor, Pharmacology, Cornell University Medical School, New York, N. Y.

Chaikelis, Alexander S., Ph.D., Assistant Professor, Biology, College of the City of New York, N. Y.

Clark, John R., Ph.D., Associate Professor, School of Education, Teachers College, Columbia University, New York, N. Y.

Clausen, Lucy W., B.S., Assistant, Entomology, American Museum of Natural History, New York, N. Y.

Crosman, Arthur M., Ph.D., Instructor, Biology, New York University, New York, N. Y.

Crum, Harry E., Columbia Carbon Co., New Yoık, N. Y.

Crunden, Allen B. Jr., M.D., Medical Center, Jersey City, N. J.

Duschak, Ernst Theodor, M.D., New York, N. Y.

Du Vigneaud, Vincent, Ph.D., Professor, Biochemistry, Cornell University Medical School, New York, N. Y.

Egloff, Gustav, Ph.D., Director Research, Universal Oil Products Co., Chicago, Ill.

Ehrmann, Martin, Gem Expert, New York, N. Y.

Erlanger, Gustav, M.D., New York, N. Y.

Etkin, William, Ph.D., Instructor, Biology, College of the City of New York, N. Y.

Fairchild, Johnson E., New York, N. Y.

Frank, Karl G., Museum of Science and Industry, New York, N. Y.

Fruton, Joseph S., Ph.D., Rockefeller Institute, New York, N. Y.

Gibson, Count D., C.E., Professor of Geology, Georgia School of Technology, Atlanta, Ga.

Greene, Marius B., M.D., Chief Medical Adviser, World Foundation Pub. Enlightenment on Traffic in Opium, New York, N. Y.

Gregory, Joseph, Ph.D., Lecturer, Zoology, Columbia University, New York, N. Y.

Guanella, Frances M., Ph.D., Psychologist, Board of Education, New York, N. Y.

Hadley, Charles E., Ph.D., Associate Professor, Biology, N. J. State Teachers College, Upper Montclair, N. J.

Hawkins, Alfred C., Ph.D., Assistant Soil Scientist, Soil Conservation Service, U. S. Department of Agriculture, N. J.

Hill, John Eric, Ph.D., Assistant Curator, Mammals, American Museum of Natural History, New York, N. Y.

Hughes, Mina M. Edison, West Orange, N. J.

Jordan, William E., B.S., Librarian, Brooklyn Botanic Garden, Brooklyn, N. Y.

King, Cecil V., Ph.D., Assistant Professor, Chemistry, New York University, New York, N. Y.

Kovar, Katherine E., Brooklyn, N. Y.

LaForge, Laurence, Ph.D., Professor, Geology, Teachers School Science, Harvard University, Cambridge, Mass.

LaMer, Victor K., Ph.D., Professor, Chemistry, Columbia University, New York, N. Y.

Larkin, Pierce, Geologist, Tulsa, Okla.

Lavin, George I., Ph.D., Chemist, Rockefeller Institute, New York, N. Y.

Levine, Michael, Ph.D., Biologist, Research Laboratories, Montefiore Hospital, New York, N. Y.

Lichtman, Frieda, Biology, New York, N. Y.

Ludwig, Daniel, Ph.D., Biology, New York University, N. Y.

Lynch, J. Joseph, A.M., Director, Seismic Observatory, Fordham University, New York, N. Y.

Merrill, Walter Scott, New York, N. Y.

Messer, Harold M., A.M., Assistant Professor, Long Island University, Brooklyn, N. Y.

Meyer, Helen, Psychologist, Yonkers, N. Y.

Michaelis, Leonor, M.D., Physical Chemistry, Rockefeller Institute, New York, N. Y.

Miller, Ralph L., Ph.D., Department of Geology, Columbia University, New York, N. Y.

Miller, Ruth A., Ph.D., Instructor, Anatomy, Columbia University, New York, N. Y.

Montgomery, Arthur, Mineralogist, New York, N. Y.

Moore, William, Ph.D., Research Entomologist, American Cyanide Co., New York, N. Y.

Narath, P. A., M.D., New York, N. Y.

Nichols, Charles K., New York Telephone Co., New York, N. Y.

Nisson, Henry W., Ph.D., Assistant Professor, Psychobiology, Yale Medical School, New Haven, Conn.

Perrine, Irving, Ph.D., Petroleum Geologist and Oil Producer, Oklahoma City, Okla.

Petty, Dabney E., Chief Geologist, Petty Geophysical Engineering Co., San Antonio, Texas.

Presner, Jacques C., M.D., Attending Urologist, Lenox Hill Hospital, New York, N. Y.

Rex, Edgar G., M.S., Supervisor, Plant Pest Control, State Department of Agriculture, Trenton, N. J.

Sargeant, Bessie B., Psychologist, New York, N. Y.

Saunders, John Richard, Assistant Curator, Education, American Museum of Natural History, New York, N. Y.

Schoenheimer, Rudolph, M.D., Assistant Professor, Biochemistry, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Shedlovsky, Theodore, Ph.D., Physical Chemistry, Rockefeller Institute, New York, N. Y.

Sherrard, Grover C., M.D., U. S. Public Health Service, U. S. Quarantine Station, Staten Island, N. Y.

Simpson, Jennie L., Ph.D., Assistant Professor, Botany, Hunter College, New York, N. Y.

Slichter, Walter I., E.E., Professor Electrical Engineering, Columbia University, New York, N. Y.

Snyder, Eric G., Ph.D., Biochemistry, New York, N. Y.

Spencer, Douglas, Psychologist, New York, N. Y.

Stone, Gordon C., Ph.G., Department of Ophthalmology, Columbia University, New York, N. Y.

Stookey, Byron, M.D., Professor, Neuro-Surgery, Columbia University, New York, N. Y.

Suarez, Ramon M., M.D., Associate Professor, Tropical Medicine, School of Tropical Medicine, Columbia University, Santurce, Puerto Rico.

Suhrie, Ambrose L., LL.D., Ph.D., Professor, Education, School of Education, New York University, New York, N. Y.

Sulzberger, Arthur Hays, B.S., President and Director, New York Times, New York, N. Y.

Swick, David A., M.D., New York, N. Y.

Taub, Abraham, A.M., Associate Professor, Chemistry, College of Pharmacy, Columbia University, New York, N. Y.

Taub, Harry, M.A., Professor, Pharmacy, College of Pharmacy, Columbia University, New York, N. Y.

Thorndike, Robert L., Ph.D., Associate Professor, Psychology, Teachers College, Columbia University, New York, N. Y.

Trainer, John A., Investment Counselor, New York, N. Y.

Vaillant, George C., Ph.D., Associate Curator, Anthropology, American Museum of Natural History, New York, N. Y.

Wertheimer, Alfred H., New York, N. Y.

Wheeler, Gerard E., Ph.D., Instructor, Geology, Rutgers University, New Brunswick, N. J.

Wood, Florence Dowden, Ph.D., Long Island City, N. Y.

Zeman, Frederick D., M.D., New York, N. Y.

Zerk, Oscar U., M.D., Chicago, Ill.

ASSOCIATE MEMBERS

Abbott, Cyril E., Geologist, Searcy, Ark.

Aberle, Sophie D., M.D., General Superintendent, United Pueblos Agency, Indian Service, Department of Interior, Albuquerque, N. M.

Agersborg, H. P. K., Ph.D., Consulting Biologist, National Park Service, Centralia, Ill.

Aherne, Vina M., New Haven, Conn.

Aitken, Robert T., M.A., Honolulu, Hawaii.

Alban, Guillermo L., Bureau of Science, Manila, P. I.

Albritton, Claude C., Jr., Ph.D., Instructor, Geology, Southern Methodist University, Dallas, Texas.

Alexanderson, Ernst Frederick Werner, D.Sc., Consulting Engineer, General Electric Co., Schenectady, N. Y.

Allen, Bennet M., Ph.D., Professor, Zoology, University of California, Los Angeles, Calif.

Allen, William F., Ph.D., Professor, Anatomy, University of Oregon, Portland, Oregon.

Allen, W. S., Ph.D., President, John B. Stetson University, DeLand, Fla.

Allison, James B., Ph.D., Assistant Professor, Biochemistry, Rutgers University, New Brunswick, N. J.

Alonzo, Agustin S., Ph.D., Dean, North Luzon Junior College, University of Philippines, Manila, P. I.

Andrews, Thomas G., Ph.D., Assistant Professor, Geology, University of Alabama, University, Ala.

Antevs, Ernst, Ph.D., Research Associate, Geology, Carnegie Institute.

Arenson, Saul B., Ph.D., Associate Professor, Chemistry, University of Cincinnati, Cincinnati, Ohio.

Armstrong, Charles, M.D., U. S. Public Health Service, Washington, D. C.

Arnold, Samuel T., Ph.D., Dean, Undergraduates, Brown University, Providence, R. I.

Arrington, Ruth E., Ph.D., Sociology, Arlington, Va.

Ashauer, Hans F., Ph.D., Coalinga, Calif.

Atwood, Wallace W., Ph.D., President, Clark University, Worcester, Mass.

Augustine, Donald L., Sc.D., Assistant Professor, Helminthology, Harvard Medical School, Boston, Mass.

Averill, Lawrence A., Ph.D., Professor, Psychology, State Teachers College, Worcester, Mass.

Bachmann, G., M.D., Professor, Physiology, Emery University, Atlanta, Ga.

Bailey, Benjamin F., M.D., Bailey Sanatorium, Lincoln, Neb.

Bailey, J. J., Geologist, Midland, Texas.

Ballon, Harry C., M.D., Montreal, Canada.

Bass, Elizabeth, M.D., New Orleans, La.

Bastiony, Jose A. Presno y, President, Academia de Ciencias de la Habana, Habana, Cuba.

Bauer, Frederick, Principal, Connecticut State High School, Kent, Conn.

Beckwith, R. H., Ph.D., Associate Professor, Geology, University of Wyoming, Laramie, Wyo.

Best, J. Boyd, Houston, Texas.

Boyle, Albert C., Jr., Ph.D., National Park Service, Jensen Uintah County, Utah.

Bradley, John H., Jr., Lowell, Mass.

Bray, A. W., A.M., Professor, Biology, Rensselaer Polytechnic Institute, Troy, N. Y.

Brinley, Floyd J., Ph.D., Associate Professor, Zoology, State College, Fargo, N. D.

Brooks, Stanley Truman, Ph.D., Curator, Invertebrate Zoology, Carnegie Museum, Pittsburgh, Pa.

Brownback, J. H., A.B., Professor, Biology, Ursinus College, Collegeville, Pa.

Bucher, Walter H., Ph.D., Chairman, Geology and Geography, University of Cincinnati, Cincinnati, Ohio.

Buckstaff, Ralph N., Oshkosh, Mich.

Burley, Benjamin T., M.D., Neurologist, Worcester City and Memorial Hospital, Worcester, Mass.

Calhane, D. F., Ph.D., Worcester, Mass.

Carroll, Robert S., M.D., Psychiatrist, Highland Hospital, Asheville, N. C.

Cartwright, Weldon E., Geologist, Shreveport, La.

Chamberlain, Ralph V., Ph.D., Professor, Zoology, University of Utah, Salt Lake City, Utah.

Clark, Frank R., B.S., Geologist, Ohio Oil Company, Tulsa, Okla.

Clinton, H. G., Manhattan, Nev.

Cole, William H., Ph.D., Professor, Physiology and Biochemistry, Rutgers University, New Brunswick, N. J.

Conkling, Russell C., Midland, Texas.

Cooper, Hershel H., M.S., Consulting Geologist, Pan-Am. Petrol. and Transport Co., San Antonio, Texas.

Corry, Andrew V., Geologist, Buenos Aires, Argentina.
Crook, Welton Joseph, D.Eng., Professor, Metallurgy, Stanford University, Stanford, Calif.
Crosby, Arthur S., Los Angeles, Calif.
Dale, E. E., Ph.D., Assistant Professor, Biology, Union College, Schenectady, N. Y.
Dale, Nelson Clark, Ph.D., Head Department, Geology, Hamilton College, Clinton, N. Y.
Dallenbach, Karl M., Ph.D., Professor, Psychology, Cornell University, Ithaca, N. Y.
Deen, Arthur H., A.M., Associate Professor, Geology, University of Texas, Austin, Texas.
Dodge, Francis D., Ph.D., Chemist, Dodge & Olcott Co., Bayonne, N. J.
Eby, J. Brian, Ph.D., Consulting Geologist, Houston, Texas.
*Echterbecker, Charles F., A.B., Professor, Psychology, State Teachers College, Worcester, Mass.
Elcock, Harry A., M.S., District Agriculturist, Amalgamated Sugar Co., Twin Falls, Idaho.
Emerson, Herbert W., M.D., Assistant Professor, Hygiene, University of Michigan, Ann Arbor, Mich.
Estes, William L., Jr., M.D., Chief Surgeon, St. Luke's Hospital, Bethlehem, Pa.
Ewing, Maurice, Ph.D., Assistant Professor, Physics, Lehigh University, Pa.
Faessler, Carl, Ph.D., Professor, Mineralogy, Laval University, Quebec, Canada.
Fessenden, G. Russell, Washington, D. C.
Field, Henry, D.Sc., Curator, Anthropology, Field Museum of Natural History, Chicago, Ill.
Freeman, Bruce C., Ph.D., Instructor, Geology, Ohio State University, Columbus, Ohio.
Glaser, Otto C., Ph.D., Stone Professor, Biology, Amherst College, Amherst, Mass.
Glass, Jewell J., A.M., Junior Geologist, U. S. Geological Survey, Washington, D. C.
Goldring, Winifred, Sc.D., Assistant State Palæontologist, State Museum, Albany, N. Y.
Grant, Ulysses Sherman, Ph.D., Chairman, Invertebrate Palæontology, University of California, Los Angeles, Calif.
Green, Frank T., Ph.G., Emer. Dean, Pharmacy, University of California, San Francisco, Calif.
Griffith, Glenn R., Geologist, Tulsa, Okla.
Grimsley, George P., Ph.D., Geological Engineer, Baltimore & Ohio Ry., Baltimore, Md.
Halbouty, M. T., Geologist, Houston, Texas.
Harvey, Samuel Clark, M.D., Sc.D., Surgeon-in-chief, New Haven Hospital, New Haven, Conn..
Headley, Joseph B., Geologist, Roswell, N. Mex.
Henderson, Homer I., Geologist, Houston, Texas.
Hollister, Joseph S., Santa Barbara, Calif.
Holm, Donald A., Geologist, State Land Department, Phoenix, Ariz.
Hotchkiss, William O., Ph.D., President, Rensselaer Polytechnic Institute, Troy, N. Y.

Hubbard, George D., Ph.D., Emer. Professor, Geography and Geology, Oberlin College, Oberlin, Ohio.

Huber, Edward Godfrey, Ph.D., Assistant Dean, Harvard School of Public Health, Harvard University, Cambridge, Mass.

Ivy, John S., B.S., President and Chief Geologist, Union Producing Co., Houston, Texas.

Jackson, G. L., Geologist, Houston, Texas.

Janovy, John, Geologist, Hauma, La.

Jillson, W. R., Sc.D., Geologist, Mid-Continent and Appalachian Oil Fields, Frankfort, Ky.

Johnson, Meredith E., Department of Conservation & Development, Trenton, N. J.

Jones, Hiram A., Director, Health and Physical Education, State Education Department, Albany, N. Y.

Keith, Arthur, A.M., Geologist, U. S. Geological Survey, Washington, D. C.

Kelly, Emerson Crosby, Albany, N. Y.

Kirkham, Virgil R. D., Ph.D., Chief Geologist and Exec. Vice-President, Pacific Exploration Company, Saginaw, Mich.

Kleinpell, Robert M., Ph.D., Geologist, U. S. Geological Survey, Bakersfield, Calif.

Kittredge, M. B., Shawnee, Okla.

Knight, Samuel H., Ph.D., Professor, Geology, University of Wyoming, Laramie, Wyo.

Lindgren, Waldemar, Sc.D., Emer. Professor, Economic Geology, Mass. Institute of Technology, Cambridge, Mass.

McGraw, Arthur B., M.D., Henry Ford Hospital, Detroit, Mich.

*Mackay, G. M. J., American Cyanamid Company, Stamford, Conn.

Makemson, Maud Worcester, Ph.D., Astronomist, Director Observatory, Vassar College, Poughkeepsie, N. Y.

Merwin, Lewis B., Bloomington, Ill.

Miller, Arthur K., Ph.D., Associate Professor, Geology, State University of Iowa, Iowa City, Iowa.

Moore, Barrington, A.B., Corfe, Taunton, England.

Murchison, Carl, Ph.D., Sc.D., Editor and Director, Journal Press, Provincetown, Mass.

Oterson, Lillian, West Haven, Conn.

Pace, Donald C., Ph.D., Instructor, Zoology, Johns Hopkins University, Baltimore, Md.

Palmer, Robert L., Geologist, Houston, Texas.

Robinson, Daisy M. O., M.D., Surgeon, U. S. Public Health Service, Washington, D. C.

Rosenholtz, Joseph Leon, Ph.D., Professor, Geology and Mineralogy, Rensselaer Polytechnic Institute, Troy, N. Y.

Sayles, Robert W., A.B., Research Associate, Geology, Harvard University, Cambridge, Mass.

Scantlebury, Ronald E., Wayne University, College of Medicine, Detroit, Mich.

Schlosberg, Harold, Ph.D., Associate Professor, Psychology, Brown University, Providence, R. I.

Schroeder, William C., Associate Curator, Ichthyology, Museum of Comparative Zoology, Cambridge, Mass.

Scofield, Carleton F., Ph.D., Associate Professor, Psychology, University of Buffalo, Buffalo, N. Y.

Shadle, Albert Robert, Ph.D., Professor, Zoology, University of Buffalo, Buffalo, N. Y.

Simmons, Lt. Colonel James Stevens, M.D., Sc.D., Assistant, Corps Area Surgeon, Army Base, Boston, Mass.

Sivickes, P. B., Ph.D., Ard. Professor, Comparative Anatomy and Embryology, V. D. Universitetes, Kaunas, Lithuania.

Sloan, Louise L., Ph.D., Instructor, Johns Hopkins Medical School, Baltimore, Md.

Thompson, Thomas G., Ph.D., Professor, Chemistry, University of Washington, Seattle, Wash.

Wisser, Edward, Geologist, Berkeley, Calif.

Wolfenden, M. E., Ph.D., Psychologist, Beverly Hills, Calif.

STUDENT MEMBERS

Beale, Helen Purdy, Ph.D., Associate, Plant Pathologist, Boyce Thompson Institute, Yonkers, N. Y.

Cady, Wallace M., Geology, Columbia University, New York, N. Y.

Chager, Felix, A.B., Assistant in Petrology, Columbia University, New York, N. Y.

Glaviano, Vincent V., Student, College of the City of New York, N. Y.

Hoberman, Henry D., A.B., Student, Biological Chemistry, Columbia University, New York, N. Y.

Noback, Charles R., M.S., Teaching Fellow, New York University, New York, N. Y.

Shupack, Dorothy, Brooklyn, N. Y.

* Deceased.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 MARCH, 1939 No. 5

SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 6, 1939

PROFESSOR PAUL F. KERR, Department of Geology, Columbia University: *A Decade of Research in the Clay Minerals.* (This lecture was illustrated by lantern slides.)

Doctor Paul MacClintock, of Princeton University, was scheduled to give an address entitled, "*Weathering of the Glacial Till in New Jersey,*" but illness prevented his attendance. Professor Kerr generously served as a substitute in Doctor MacClintock's stead and delivered an excellent lecture, title as given above. No abstract was received.

TRANSACTIONS of the New York Academy of Sciences, Series II, Volume 1, No. 5, March, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

SECTION OF BIOLOGY

February 13, 1939

Professor Lorande Loss Woodruff, Professor of Protozoölogy, Yale University: *Some Pioneers in Microscopy, with Special Reference to Protozoology.* (This lecture was illustrated by lantern slides.)

Although the science of optics formally starts with Euclid, there seems to be no evidence that the possibilities of a lens as an optical instrument were appreciated until toward the close of the thirteenth century. Roger Bacon suggested the value of lenses for persons with weak eyes, but apparently made no significant advance in the theory or the use of lenses. The actual invention of spectacles followed shortly after and their use gradually spread over Europe during the fourteenth and fifteenth centuries.

Then Leonardo da Vinci considered lenses, and the sixteenth century saw their optical properties studied in particular by Leonard Digges and Francesco Maurolico, while Giambattista della Porta made the earliest significant suggestion of a bilenticular system for the magnification of small objects. Near the turn of the century came the first feeble impact of lenses on biology at the hands of Thomas Mouffet, George Hoefnagel, and Fabio Colonna, just as the compound microscope was about to be invented by Zacharias Jensen. Galileo in 1610 was the first to use the instrument effectively and he introduced it to his colleagues in that coterie of scholars, including Cesi, Colonna, Stelluti, Fontana, Faber and Porta, meeting as the Accademia dei Lincei. The first figures ever made with the aid of a compound microscope to appear in a printed book were by Francesco Stelluti in 1630.

Probably the most important gleanings with the microscope during the next half century were those by Giambattista Hodierna and Francesco Fontana, while the instrument was exploited by Athanasius Kircher without giving detailed

descriptions or figures of anything that it had revealed to him. Then a gifted physician of Paris, Pierre Borel, made a long series of observations and published in 1656 the first volume solely devoted to microscopy. And furthermore, he employed the instrument in his profession; probably the first practical use of the microscope in medicine. But an Englishman, Robert Hooke, was the first to realize to the full the importance of using instruments which increase the powers of the senses in general and of vision in particular, and to express it convincingly in 1665 in a remarkable book: *The Micrographia.* Here he described and emphasized for the first time the "little boxes or cells" of organic structure and indelibly inscribed the word *cell* in biological literature.

The Micrographia had an immense influence on microscopy, gleanings from its wealth of plates "embellishing" technical and popular manuals on the microscope for upward of a century. When the book was being written, Samuel Pepys purchased a microscope and thought five pounds, ten shillings "a great price for a curious bauble." And it is not recorded that Hooke's demonstrations, or those of the contemporary London physician, Henry Power, changed his opinion, but from this time on magnification was firmly established as an indispensable aid in biological research. The so-called classical period in microscopy unfolds during the latter part of the seventeenth century with the outstanding contributions of Malpighi, Grew, Swammerdam, and Leeuwenhoek—the latter the founder of protistology.

Antony van Leeuwenhoek enlivened every drop of water by his placid observations with simple microscopes made by his own hands. In 1674, while examining some pond water, he discovered, as he said, "very many little animalcules . . . the motion of most of these animalcules in the water was so swift, and so various, upward, downwards, and round about, that 'twas wonderful to see; and I judge that some of these little creatures were above a thousand times smaller than the smallest ones I have ever yet seen, upon the rind of cheese, in wheaten flour, mould, and the like." Two years later he gave

the first description of an identifiable Protozoon and also discovered Bacteria in pepper-water. He says, "the fourth sort of little animals . . . were incredibly small; nay, so small, in my sight, that I judged that even if one hundred of these very wee animals lay stretched out one against another, they could not reach to the length of a grain of coarse sand." Later he discovered several parasite Protozoa in man and beast, and Bacteria in the human mouth. Of course Leeuwenhoek made many other discoveries during his long life—his studies were not confined to animalcules—but it is enough that he is justly regarded as the Father of Protozoölogy and Bacteriology.

Leeuwenhoek complained: "I ofttimes hear it said that I do but tell fairy-tales about the little animals," but confirmation soon came. Hooke demonstrated animalcules in pepper-water at a meeting of the Royal Society in 1677, and the following year wrote that he had "discovered vast multitudes of those exceeding small creatures which Mr. Leeuwenhoek had described. . . . I was very much surprised at this so wonderful a spectacle, having never seen any living creature comparable to these for smallness: nor could I indeed imagine that nature had afforded instances of so exceedingly minute animal productions." The same year Christiaan Huygens described several animalcules familiar to Leeuwenhoek, in a letter which, indirectly, soon reached him but was not published until recently. Then thirteen years later Bonanni's *Observationes circa Viventia* appeared with the first published figures of ciliates, and was shortly followed by a series of papers in the Philosophical Transactions: King's *Several Observations and Experiments on the Animalcula in Pepper-water,* Harris's *Some Microscopical Observations of Vast Numbers of Animalcula Seen in Water,* Gray's *Several Microscopical Observations and Experiments,* and an anonymous contributor's letter which says, "I know many question the sincerity of his (Leeuwenhoek's) relations, but I can do him the right to affirm that as far as I am able to follow him, and I have tried many of his experiments, I find him always faithful in matter of fact, and therefore question not his veracity in other things."

So under Leeuwenhoek's stimulus by the turn of the century the world had become conscious of a microcosm below the limits of unaided vision. Then in 1718 appeared the first special treatise on the Protozoa and other tiny organisms by Louis Joblot of Paris. This is a remarkable and curious book that describes new microscopes and many new forms of animalcules, and makes the first general attempt to give the latter appropriate names. Furthermore, in the study of the origin of the organisms, Joblot was the first to boil infusions in order to eliminate life; a method exploited in studies on biogenesis by Spallanzani and others a half century later without a thought of Joblot.

The next significant contribution was made by Dr. John Hill in 1752, who gave the first formal classification of animalcules, including them in the Animal Kingdom as the most primitive group. He says that he "arranged them into a regular method, and gave them denominations"—one the now familiar *Paramecium.* Linnaeus in the twelfth edition of his *Systema Naturae* grouped all of them under three genera: *Volvox, Furia,* and *Chaos,* and all the infusoria in a single species, *Chaos infusorium.*

Obviously an immense field awaited intensive study, and this was begun in a desultory way by many amateur and professional biologists during the closing decades of the eighteenth century, the outstanding contributions being made by O. F. Müller in 1773 and 1786. And then over a half century passed before Ehrenberg in 1838 and Dujardin in 1841 afforded a sufficiently broad view of these "simple" animals to justify the establishment of the phylum Protozoa by von Siebold in 1845. Thus the development of Protozoölogy exemplifies the dictum of an early microscopist that "the likeliest method of discovering the truth is the observation of many on the same subject," but surely none of the pioneers even dreamed that studies by their twentieth century disciples would indicate that there are more species of Leeuwenhoek's "little animals" than comprise all the rest of the Animal Kingdom.

SECTION OF PSYCHOLOGY

February 20, 1939

Doctor Clark L. Hull, Institute of Human Relations, Yale University: *Modern Behaviorism and Psychoanalysis.*

Behaviorism and psychoanalysis are generally regarded as independent disciplines, yet both concern the behavior of the same organism—man. Presumably the same ultimate laws underlie all of human behavior. When these are known and their implications fully elaborated we shall have an integration of the social sciences. A major task in such a development will be to integrate whatever truth lies in behaviorism and psychoanalysis. Moreover, the integrational process will tend powerfully to fill in gaps and correct errors in both disciplines. The first step in this integration is to identify the numerous equivalent concepts and behavior principles existent in the two disciplines but now hidden under differences in terminology. The present paper presents a tentative and partial statement of such equivalences.

Perhaps the most primitive and basic notion in psychoanalysis is the *id*. To a behavioristic analysis this concept appears to be the basic physiological source of drives together with a reification of the fact that conditioning or reinforcement occurs, presumably according to the "law of effect."

A central and closely related concept of psychoanalytic theory is that of *libido*. In a behavioristic system this appears to correspond to a generalized or undifferentiated drive with special emphasis on "pleasure." The extent to which the various drives recognized by academic psychology are actually generalized remains largely unexplored. We do not know, for example, to what extent habits set up on the basis of one drive will function on the basis of others; nor to what extent satiation of one drive will weaken the others.

Closely related to the concept of the id and of the libido is that of *object cathexis*. Cathexis appears to be the result of a

conditioning process whereby organisms acquire the tendency to seek or strive to attain goals or subordinate goals. Object cathexis may now be derived quite nicely from behavior theory. Involved in this derivation is the principle of response equivalence, *i.e.,* alternative reactions leading to the same goal (reinforcing state of affairs).

Behavioristic analysis indicates not only that there is a considerable variety of objects which will mediate reinforcement of acts leading to the satiation of the various drives, but that these objects present a natural hierarchy in respect to their reinforcing potentiality. It is also to be noted that at the different ages of childhood there is a progressive enlargement in the number of objects which are physically and socially available for libidinal reinforcement. It thus comes about that while the objects sexually cathected are to a considerable extent a matter of chance, there is nevertheless on the average a definite tendency to an orderly sequence in object cathexis. Thus it is inevitable that if sexual cathexis in an infant occurs, it will be to its own body; at this age sexual activity can only take some very simple form of masturbation such as thigh rubbing. This type of sexual goal seeking necessarily becomes centered in the subject's own body; when generalized, psychoanalysis calls the resulting habit organization *narcissism.*

As the child grows older chance may bring it about that the object cathected is a playmate of the same sex; thus naturally would arise one type of *homosexuality.* But a person of opposite sex usually has a higher place in the hierarchy of reinforcing agents than one of the same sex. It thus comes about that, except for anxiety potentials, the person usually cathected in the end is a person of opposite sex; this results in *heterosexuality.* However, inanimate objects may be cathected, in which case we have *fetichism.* In some Russian breeding experiments, stallions have been taught to copulate with an inanimate apparatus for which they will strive violently. A more complex situation is that in which a man will have a sexual craving to possess a bit of feminine underwear. This shifting

from one sexual goal object to another is known in psychoanalysis as *transference* of the libido.

It sometimes happens that the conditioning to a given goal object may be too strong; animal experiments show that this may interfere with the conditioning to a new goal object. Thus arises one form of what psychoanalysis calls a *fixation.* Extreme fixation prevents further transference. But even if the organism has transferred or shifted upward through the hierarchy of goal objects successfully, animal experiments seem to show that if a severe frustration or emotional shock occurs, the organism may revert to a previously abandoned type of goal. In psychoanalysis this is called *regression.* Sometimes, however, the emotional shock may be so profound that its generalized effects extend from the goal in which the trauma occurred to practically all such goals; this is known in psychoanalysis as one form of *repression.*

In the process of trial-and-error characteristic of life, two rather different types of action are followed by reinforcement. Approach behavior is one such type. When generalized in social situations this is called *love.* In other (injurious) situations the separation of the organism from the situation is reinforcing. This separation may be effected by (1) the flight of the organism or (2) by the removal of the critical element in the situation. The state of the organism in the former situation is called *fear.* Vague, persisting anticipatory fears are called *anxiety.* The organism learns by trial-and-error that other (offending) organisms will remove themselves when attacked: thus *aggression* is reinforced. Persisting impulses to aggression are called *hate.* Sometimes through previous conditioning, the same person may have an excitatory potentiality for both love and fear or for both love and hate: psychoanalysis calls this *ambivalence.*

When we have no functioning verbal reactions conditioned to a past situation or present intent (r_G) we obviously cannot tell about it. Such events and motivations are said by psychoanalysis to be *unconscious,* whereas when we can tell about

them they are said to be *conscious*. Since children have few words before three or four years of age, it is inevitable that they should not be able to tell about events of that period, yet lasting habits, including toilet training, are formed at this time. These events are naturally "unconscious."

Through the "law of effect" the organism learns by trial-and-error that words originally used in communication may be used as *pure stimulus acts* to mediate purely individual problem solution. The complex organization of symbolic habits for the purpose of thought and reasoning appears to be substantially what psychoanalysis means by the term *ego*. Since this is all verbal it follows (1) that the ego is highly conscious, and (2) that it is highly intelligent (as contrasted with the unconscious).

In the early years of a child's life, much of conduct is usually motivated through punishment or threats on the part of parents and teachers. Partly because of the early age when these habits were set up, they are not verbalized. For this reason the subject usually can not tell why such acts are performed or why contrary acts give rise to anxiety. The mass of such habits apparently makes up what psychoanalysts call the *super-ego,* and what usually is known as conscience. The anxiety arising from the violation of such habits we call the "pricking of conscience." The lack of verbalization of such habits seems to be the substance of what is known as *internalization.*

In the disciplinary situation a person usually learns to escape punishment for previous acts by ingenious word sequences which may not correspond to fact but nevertheless are often reinforced according to the "law of effect." In a similar manner by trial-and-error a person may by a certain word sequence relieve himself of anxiety occasioned by an act in violation of the super-ego. The word sequences may be contrary to the logic of the ego system but its subvocal execution will be reinforced, nevertheless: this is called *rationalization* by psychoanalysis.

As a last conceptual equivalence we take the case where a person with a heavy load of anxiety finds through trial-and-error that it is reduced, though but temporarily, when he performs some special act. This act may be appropriate enough where it first occurred. Nevertheless the law of effect will reinforce it and it will continue to recur in all sorts of inappropriate situations as well. The patient's acquaintances will regard him as strange or neurotic, and the odd behavior will be regarded as a neurotic symptom, as indeed it is. This is a behavioristic rendering of perhaps the most important single psychoanalytic principle, that of *symptom formation.*

SECTION OF ANTHROPOLOGY

February 27, 1939

Doctor Herbert J. Spinden, Curator, American Indian Art and Primitive Cultures, Museum, Brooklyn Institute: *The Archaeology of the Northern Andes.* (This lecture was illustrated by lantern slides.)

In the hope of finding evidence that a cultural highway ran in ancient times from Mexico to Peru—from one area of really high civilization in aboriginal America to the other—I spent nearly six months in traversing Colombia, Ecuador and Peru under the auspices of the Brooklyn Museum. The plan was to enter little known but strategically located regions off the main routes of travel, making photographs, drawings, etc., of the archaeological objects found in private cabinets and local museums, also visiting such ancient sites as could be reached without great loss of time.

Actually the lion's share of attention went to Colombia, a large and diversified country that archaeologically has been reported on in a very imperfect way. Bogotá, its capital—which last year celebrated its four hundredth anniversary as an outpost of Europe and which before 1538 was the head of the Chibcha state—lies about midway between those two famous lakes of Texcoco and Titicaca on whose shores flourished the Aztecs and the Incas before their careers of empire were likewise cut short by the Spanish conquest. A stretch of four thousand miles of rough mountain trail separates the two ends of this archaeological quest. Whereas, only a few years ago we despaired of finding definitive evidence of broadcast exchange in ideas and products over this really great distance, today the final proof seems well within our grasp. Several important techniques in art are now known to be distributed over this long trail, such as:

(1) Painting with wax and with black and red pigments to give negative designs on pottery.

(2) Covering surfaces of gourds and of wooden and pottery vessels with a sort of lacquer in bright inlaid colors.

(3) Covering objects of carved wood, of shell, bone, etc., with an inset mosaic of bits of jade, jasper, turquoise, vermillion colored shells, etc.

(4) The handling of several metals by the lost wax method of casting and the filigrane method of designing.

(5) The use of flat and cylindrical stamps for transferring designs to the body or to textiles.

(6) The use of marcasite mirrors, of clay whistles with four notes, of large conch trumpets, of looms and spindle whorls, etc., etc.

All of these technological concepts can be demonstrated as passing from Mexico to Peru. Here we are only on the threshold of our studies; for motives in religion and art have a similar distribution down the Sierra Madre and down the Andes: the Sky God as a jaguar; the Sun as a disk with serpent rays and containing the face of this jaguar god; the humanization of sea snails, crabs, centipedes, scorpions, etc., as patrons of various special abilities; the humanization of eagles and several other animals as the patrons of warrior societies making head-hunting warfare. All of these and numerous motives in decoration, numerous domesticated plants, now constitute a mass of visual evidence documenting the flow of ideas along the mountain road from Mexico to Peru. This interchange over such a great distance was made through numerous self-governing tribal societies each with its own stamp and personality, its own language and its own arts and ceremonies.

Special attention will be paid to several culture areas, especially in Colombia. I will not do more than briefly mention the Tairona culture area in the vicinity of Santa Marta, my port of entry, and the locality in which my investigations began. Here several cemeteries have been excavated by J. Alden Mason, the results indicating a community of ideas with parts of western Venezuela. Nor will I devote much time to the

Chibcha, of the high central plateau, or to the Quimbaya, on the middle course of the Cauca, for their products have long been well known, thanks to the works of Vincente Restrepo and his son, Ernesto Restrepo Tirado, to mention only two of the scholars of Colombia.

I do wish, however, to direct attention to the region between Cartagena and the Gulf of Urubá, the seat of three little kingdoms or queendoms called Zenu, Panzenu and Zenufana, occupying the region of the Sinu River and of the San Jorge to the south. This was the first part of the American continent to feel the impact of Spanish arms. From the Sinu area was taken the heaviest loot of the New World and by 1530 practically all the cemeteries had been dug up by Spanish armies for the golden treasure buried with the dead. The few specimens of ceramic art which have survived are really superb and the same can be said of surviving examples of their gold jewelry.

The culture of Sinu may have been sybaritic and ultrafeminine for two of their governments had queens. In the ceramic art women are delightfully and smilingly portrayed and the female breast is represented in lobed pots, etc. According to the stories told by the first Spanish adventurers women rulers of the Sinu had rich palaces and had a special manner of climbing into their hammocks. These were supported by carved poles covered with gold. When the time came to retire a living ladder was formed on the bodies and shoulders of ladies in waiting.

The cemeteries of the Zenu were high earthen mounds over platforms and they were called "piru," clearly the origin of the present name "Peru." I visited a site near Lake Betanci south of Monteria where graves were opened some 25 years ago. In one grave 27 pounds of gold was found, and in others lesser amounts making up a total of about 100 pounds for the two mounds of Maracaybo and Flamenco. Burial seems to have been by cremation and both the jewelry and the bones were burned or blackened with fire. A certain Gusimindo

Montoya, a huaquero, had found these surviving tombs, selling the gold as metal.

Our professional pot hunter is represented in Colombia by the *huaquero,* a person who deals in *huacas,* the latter being the Peruvian word for an Indian grave and any object that comes out of it. Of course the huaquero is really looking for gold and all too frequently he finds it and melts it down. This is an ancient and officially honorable profession for the first huaqueros were the first Spaniards who landed in Colombia. According to the Colombian mining law it is permissible to make a gold claim on any ancient Indian site—*patio de Indio,* they call it—whether on public or private land. A double tax must be paid on such treasure trove.

Very different kinds of archaeological remains are found in the middle portion of the valley of the Magdalena. There are cylindrical funeral urns sealed with asphaltum on the tops of which are male and female figures modeled in a style that may be called late Archaic. No gold is found in these tombs which are obviously of earlier date than the Sinu mounds. We are concerned with secondary burial for the bones are fragmentary even in the sealed urns and the skulls are stuffed full of the small finger and toe bones, a condition which could not occur with primary burial. We are confronted with an aspect of the American Indian cult of the dead of exceptional interest the meaning of which can only be inferred. For instance there is no label instructing that the "bones be reassembled and the personality restored according to the sculptural sample."

In recent years the Colombian government has itself fostered important archeological research especially at the famous site of San Augustin with its many remarkable statues and in the closed valley of the Paez River called Tierradentro—the Inside Country—where rock-cut tombs with columns, niches and with painted walls and ceilings have been brought to light. Stone statues have also been found here as well as noble examples of gold work such as a gold mask representing a jaguar's face. This Colombian research has extended over

the Chibcha territory and into a new archeological province called Calima, centering around the site of the original Cali. Here pottery and gold work sufficiently distinct from that of the Quimbaya has been unearthed. The gold work of this Calima culture, which extends as far south as Popayan, shows a composite animal subject probably to be interpreted as a Sky God. In a collection of Guillermo Valencia, the dean of Colombian poets, there are remarkable figures in clay showing such a god seated on an elaborate stool.

Beginning with Pasto we enter a province of archaeology extending well into Ecuador. Here are found magnificent examples of negatively painted urns with designs that picture the disk of the sun and also the jaguar among the stars.

Finally I refer to the culture of Esmeraldas in northwestern Ecuador. The archaic technique of modelling figurines here appears almost as it does in Mexico. Later and more developed is a rich art with unmistakable Maya contacts. Here the animal god with a human face peering out of the open mouth is clearly seen, this being one of the outstanding conventions of Central America. Access to the humid coastal region of Esmeraldas can be had from the flanks of the Andes by the now unhealthful valley of the Patia. In Ecuador and Peru it seems probable that various parts of the coast must have received their first civilized population from the inter-Andine valleys. To the north in Colombia it seems to me that the Cauca was a more important highway than the Magdalena.

SECTION OF PHYSICS AND CHEMISTRY

February 24 and 25, 1939

Conference on "Kinetics in Solution"

The Section of Physics and Chemistry held a "Conference on Kinetics in Solution," the second of a projected series. Professor Victor K. LaMer, Columbia University, was in charge of this meeting as Conference Chairman.

The program consisted of the following papers:—

"The Effect of Solvents on Reaction Rates," by Professor Henry Eyring, Princeton University.

"Relation of Temperature Coefficients in Solvents at Constant Composition to that in Solvents at Constant Dielectric Constant," by Professor J. C. Warner, Carnegie Institute of Technology.

"Prototropy and Deuterotropy in Pseudo Acids," by Professor S. H. Maron, Case School of Applied Science.

"Kinetics of Oxygen Exchange Reactions," by Doctor Irving Roberts, Columbia University.

"Acid-Base Catalysis," by Doctor F. H. Westheimer, Chicago University.

"Kinetics of Walden Inversion," by Professor L. P. Hammett, Columbia University.

"Ionization Mechanism of Solvolytic Reactions," by Doctor Paul D. Bartlett, Harvard University.

The third Conference in this series will be held on April 14 and 15, on the subject of "Dielectrics," and will be under the guidance of Professor C. P. Smyth, Princeton University.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 | April, 1939 | No. 6

SECTION OF GEOLOGY AND MINERALOGY

March 6, 1939

Doctor Edwin H. Colbert, American Museum of Natural History: *The Migrations of Cenozoic Mammals.* (This lecture was illustrated by lantern slides.)[1]

Much of our present knowledge concerning the past distribution and migrations of land mammals is due to the long years of unexcelled research by the late William Diller Matthew, perhaps the greatest of the authorities who have dealt with this fascinating subject. Matthew's definitive essay "Climate and Evolution," originally published in the Annals of the New York Academy of Sciences in 1915, has for many years been the standard reference work on this topic, so that any discussion having to do with the migrations of mammals in past geologic time, and the distribution of mammalian life as the result of such migrations, must of necessity rest to a large extent on the principles so clearly enunciated by Matthew in the above mentioned publication. Fortunately, "Climate and Evolution," together with other papers by Matthew dealing with the

[1] Doctor J. B. Mertie, Jr., of the United States Geological Survey, was scheduled to give an address entitled, "*Geological Features of Alaska,*" but illness prevented his attendance. Doctor Colbert generously served as a substitute.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 1, No. 6, April, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

same subject, is soon to be republished by the New York Academy in a Memorial Volume, the first of the new series of Special Publications.

At the present time the land mammals of the world are distributed through five great zoogeographic realms, or regions, which are:

1. The Holarctic Region—Eurasia, north of the Himalayas and the Tibetan Plateau; and North America, including a part of Mexico.
2. The Oriental Region—India and Indo-China; many of the East Indian Islands.
3. The Ethiopian Region—Africa and Asia Minor, south of the tropic of Cancer.
4. The Neotropical Region—Central and South America.
5. The Australian Region—Australia and some of the nearby East Indian Islands.

This present-day distribution of land mammals, and incidentally of many other kinds of land life, is attributable to three controlling factors, which have operated through past geologic ages. These are:

1. The original centers at which mammalian orders, families, genera and species had their beginnings.
2. The migration of mammals from these centers of origin.
3. The extinction of mammals subsequent to their migrations.

When we look at a map of the World, as shown by a North Polar projection, it at once becomes apparent that the Holarctic Realm or Region lies within a centrally located land mass, from which radiate the other land masses, containing the remaining zoögeographic regions. It is to be noticed, also, that the land masses most closely connected with this central Eurasiatic-North American block, are those of Africa, including the Ethiopian Zoögeographic Region, and of India and the East Indies, containing the Oriental Zoögeographic Region. South America, which encloses the Neotropical Region, and Australia, in which is included the Region of that name, are more or less completely separated from the central land mass.

Now it is an interesting fact that the discoveries and studies of early Cenozoic mammals, which have been made during the past fifty years or more, go to show that a preponderant number of mammalian groups—at least in the larger sense of the word—have had their origin in the centrally located, Eurasiatic-North American land mass. That is, a majority of mammalian orders, families and genera are of Holarctic origin. Thus we should expect the present distribution of land mammals to be in part dependent on this fact.

It is evident that the distribution of mammals as affected by the factors of origin and migration is governed to a great extent by the relationships of the continental land masses during past geologic times. Matthew demonstrated very fully that the facts of origin and migration of Cenozoic and Recent land mammals may be explained in accordance with the theory of isostasy, whereby it is supposed that the continental land masses were distributed in past geologic ages much as they are today and that such land bridges or waterways as existed were due to minor vertical oscillations of epeirogenic proportions. Indeed, it is difficult to see how the distribution of land life can be satisfactorily explained in the light of any other theory.

In considering the migrations of mammals from their centers of origin, across and between the continental land masses which were oscillating vertically, but not drifting horizontally, certain principles affecting such migrations must be kept in mind. The first is that increasingly progressive forms arise at the center of origin, due to the necessity of becoming adapted to changing environmental conditions, and push out radially from this center, driving the more conservative and primitive types to peripheral regions. The second is that mammals, for the most part, are freely moving animals, capable of spreading at extraordinarily rapid rates if conditions are favorable, so that migration often times may be virtually instantaneous from the standpoint of geologic time. A third principle is that those mammals which do migrate rapidly, are apt to be progressive types, since their migration is intrinsi-

cally causative and not the result of pressure from still more advanced forms.

These principles of mammalian migration may be well illustrated by the evolutionary development and the past migrations of the Equidæ. This group of mammals was, until the close of the Pleistocene, primarily of North American development, for the horse went through the entire gamut of its evolutionary history on this continent, reaching other sections of the globe by several great intercontinental treks during later Cenozoic times. Thus, during the Miocene period the anchitheriine horses, having their origin from the North American Oligocene genus, *Mesohippus,* spread from the original Nearctic center or origin throughout the Palearctic, or Eurasiatic region. Then, at the beginning of the Pliocene, the genus *Hipparion,* originating in the North American area from a Miocene ancestor, *Merychippus,* spread from its original home to almost all sections of Eurasia, the Orient and Africa. And finally, at the beginning of the Pleistocene, *Equus,* again of North American ancestry, having its origin in the upper Pliocene genus *Plesippus,* pushed to the west to populate Eurasia, the Orient and Africa, and to the south to spread through the South American region.

Migrations such as these may have great significance with regard to intercontinental correlations. This is particularly evident in the case of the Siwalik beds of India, and their relationships to upper Cenozoic strata in other parts of the world. The Siwaliks are, broadly speaking, divisible into three groups, the Lower Siwaliks, often regarded as Vindobonian, or middle to upper Miocene in age, the Middle Siwaliks, often considered as Pontian, or lower Pliocene in age, and at the top the Upper Siwaliks, generally placed at the boundary of and bridging the gap between the Pliocene and the Pleistocene. These correlations are based on the general aspects of the several Siwalik faunas.

But recent discoveries have shown that *Hipparion* is present in the Lower Siwalik fauna. If this genus first appeared

in the New World at or near the beginning of the Pliocene, then its subsequent appearance in the Lower Siwalik fauna must indicate that this fauna is of a later age than it was once thought to be, namely of approximate Pontian rather than Vindobonian affinities. Therefore, the Middle Siwalik fauna must be of post-Pontian age. Likewise, the sudden appearance of *Equus* in the Upper Siwalik beds must be indicative of the Pleistocene age of these deposits, since *Equus* seemingly has its first appearance at the beginning of the Pleistocene in North America.

In view of the foregoing remarks, it is interesting to make a brief survey of the Pleistocene mammals of the Holarctic region, to see what factors have affected the distribution of these forms. In the first place, it must be recognized that the Pleistocene distribution of Holarctic mammals is due, to a large degree, to the heritage of their Pliocene distribution. That is, most of the Pleistocene mammals of the Holarctic region have descended in the same areas in which they are found from Pliocene ancestors. But certain notable exceptions are to be seen. Perhaps the most outstanding of the Pleistocene mammals which have transgressed the areas of their Pliocene heritage are the horses, the camels, the elephants, and the cattle. Of these, the two first named are of North American origin, spreading at the beginning of the Pleistocene to Eurasia, while the latter two are of Old World origin, spreading at the beginning of the Pleistocene to North America. Indeed, the simultaneous spread of these four types of mammals throughout a greater part of the world has often been suggested as a valid criterion for the opening of Pleistocene times, particularly in those areas which show neither glacial nor pluvial phenomena. Yet there have been other extensive migrations during the Pleistocene, that have affected the constitution of Holarctic faunas. The movement of certain lagomorphs, of the bears and of man from the Old World to the New (in addition to that of the elephants and cattle) may be cited. And in addition, there was a mass migration of large edentates from

South America into North America during Pleistocene times, thereby adding an exotic element to the typical northern fauna.

On the other hand, the non-migrations of certain Pleistocene forms are difficult to explain. For instance, why did not the woolly rhinoceros of Eurasia accompany the hairy mammoth into the New World? Certainly it would seem that any conditions that might favor the movement of one of these animals would be conducive to the migration of the other.

Finally, the factor of extinction has worked in conjunction with those of origin and migration to make the faunal regions of the world what they are today. Leaving out of account past extinctions of Tertiary times, and considering only the extinctions that decimated the ranks of Pleistocene faunas, it becomes apparent that this factor has had a great deal to do with the characterization of North American and Eurasiatic mammalian assemblages. In the Old World there was a considerable degree of extinction at the end of the Pleistocene consequent upon the final retreat of the glaciers and the gradual establishment of neolithic and subsequent cultures in that area. In North America the extinction would seem to have been more extensive and probably more sudden. Recent evidence would seem to show that numerous typical Pleistocene mammals persisted in North America until post-Pleistocene or sub-recent times. Of these there may be mentioned specifically the mammoth, mastodon, horse, camel, royal bison, ground sloths, and *Tetrameryx*. These animals were all living when man reached this continent, ten, fifteen or twenty thousand years ago. Suddenly they were wiped out. Was this remarkable extinction of such large and numerous forms due to the advent of a new, aggressive animal, Man, who in some way may have upset a delicate ecological balance?

SECTION OF BIOLOGY

March 13, 1939

Doctor John Eric Hill, American Museum of Natural History: *The Geography of African Rodents.* (This lecture was illustrated by lantern slides.)

Africa lies chiefly in the tropics, more so than any other continent, but has a wide variety of climate due to various elevations, distances from the sea, and oceanic currents. Life zones based on temperature are not evident in Africa; the correlation between mammalian and bird faunal areas are primarily with rainfall and vegetation.

The faunal divisions proposed by Wallace and others have been considerably modified by students of African Zoölogy, and the summation and map by Dr. James P. Chapin of the American Museum, Department of Ornithology (Bull. Amer. Mus. Nat. Hist., 65, 83–93), can be used as a basis for mammalian geography with but few modifications.

Africa, north of the central Sahara Desert, is predominantly Palearctic, closely related faunally with southern Europe and Asia Minor. Three divisions of this may be noted: the Atlantan, including temperate North West Africa; the Egyptian, including the lower Nile Valley; and the Saharan, the northern three-fourths of that desert.

Africa, south of the Sahara, forms the Ethiopian Faunal Region, and is divided into West and East African Subregions. The West African Subregion includes most of Africa west of the Rift Valley, between 10° North and 10° South Latitudes. The rest of the Ethiopian Region, roughly between the Sahara and the Congo, from Senegal to the Red Sea, east of the Rift Valley and south of 10° South Latitude, forms the East African Subregion.

Madagascar has very little in common faunally with Africa.

The rodent fauna of Africa is very rich, and relatively distinct. Except for the Palearctic region in the north few genera

are found both in Africa and elsewhere. At least eleven families are represented, and five of these are at present found only in Africa: Anomaluridæ, Pedetidæ, Ctenodactylidæ, Thryonomyidæ and Bathyergidæ. The Muridæ are world-wide; the Sciuridæ are absent only from the Australian and Oceanic Regions. The Myoxidæ, Spalacidæ, and Hystricidæ are Palearctic and Oriental as well as African; and the Dipodidæ are Palearctic.

Subfamilies peculiar to Africa are: Xerinæ, African ground squirrels; Lophiomyinæ, maned rats; Dendromurinæ, tree-mice; Otomyinæ, swamp-rats. The tree squirrels belong to a group found also in the Oriental Region and many of the murine groups have relatives in that region.

The African tree squirrels as a group are found throughout Africa south of the Sahara desert, except in the most arid parts of the Somali District and in the South West Arid District. No one genus is found through this area, and all except *Paraxerus* are West African, either exclusively or extending into a few marginal districts of the East African Subregion. The Ethiopian ground squirrels are found in the drier districts: the Atlantan District, the Sudanese Savanna from Gambia to Eritrea, the Somali Arid District, parts of the Eastern Veld, and the South West Arid District.

The Anomaluridæ, including the aberrant Idiurinæ, have been found nowhere outside of Africa. Two species of *Anomalurus* occur in the Eastern Montane District; the others are all confined to the West African forest and to parts of the savanna districts. Six species occur in the Guinea Forest, of which three are restricted to it. The Congo and Cameroons Forest have seven species in common. The small *Zenkerella insignis*, the sole form without gliding membrane in the family, is found only in the Cameroons District.

The Pedetidæ, with the single genus *Pedetes*, the springhaas or jumping hare, is found only in Southeastern, Rhodesian and East African Veld Districts.

The Myoxidæ, dormice, are found throughout Africa, except in the Egyptian, Saharan, and Abyssinian Highland Districts.

The Spalacidæ get into northeastern Africa. *Spalax,* the northern mole-rat, occurs only in Egypt, but *Tachyoryctes,* the bamboo-rat, occurs in the Eastern Montane and High Veld Districts.

The Muridæ, mice, are represented in Africa by a great many forms.

The Microtinæ, Holarctic in distribution, occur only in the Atlantan District in Africa.

The Cricetinæ is represented by a single genus in the Southeastern Veld and in the southern part of the Eastern High Veld.

The Lophiomyinæ, maned rats, with cricetine teeth and unique skull, occur in East Africa from the Sudan to Tanganyika.

The Gerbillinæ, jumping rats of cricetine relationships, are found throughout Africa except in the forest and mountain districts. The most extreme genera of the subfamily and the largest number of genera are found in Africa, which may be the center of origin for this group.

The Murinæ, true mice, are found throughout Africa. The genera or groups of related genera are rarely confined to a single Subregion, although none except *Rattus* are found through all the districts.

The Dendromurinæ, with reduced cusp pattern on the molar teeth, are chiefly of the East African Subregion. Four genera extend into the West African Savanna Districts, and two genera are restricted to the Congo and Cameroons Forest Districts respectively.

The Otomyinæ, with large third molars bearing many lophs, are chiefly South African, but the typical genus extends through much of the Eastern Subregion and as far into the Western as the Cameroons. Several genera live in the dry bush of South West Africa.

The Dipodidæ, jerboas, are Palearctic, but extend into the dry Sudanese Savanna and the Somali Districts.

The Ctenodactylidæ, African rock-rats, are represented by different genera in the Atlantan, Saharan, Western Sudanese

Arid, Somali Arid, and South West Arid Districts. A Pliocene species of *Pectinator* is known from India.

The Thryonomyidæ, cane rats, are confined to the Ethiopian Region, where they are found in all districts except the Abyssinian Highland, Somali and South West Arid.

The Bathyergidæ, African mole-rats or blesmols, are chiefly South African, but peculiar genera are found in the Somali and Eastern Veld and Lowland Districts, and one genus reaches Togo and the Sudan. A bathyergid is known from the Oligocene of Mongolia.

The Hystricidæ, Old World porcupines, are represented by two genera. *Hystrix* is found throughout Africa as well as in the Oriental and Southern Palearctic. *Atherurus*, confined to West Africa in the Ethiopian Region, is found also in the Oriental.

Rattus rattus, the black rat, and *Hystrix* are found practically throughout Africa. Only one other rodent is found in all districts of the Ethiopian Region, *Mastomys*, the African hut-mouse. A few genera are confined to single districts, and in such cases the district is usually of extreme climate. The moist Guinea Forest has two endemic genera: *Myrsilus*, a tree squirrel, and *Dephomys*, a tree rat. The Cameroons Forest has two restricted types: *Zenkerella*, the scaly tailed mouse-squirrel, and *Prionomys*, a tree mouse. The Congo Forest has one genus, *Deomys*, a large tree mouse. The Abyssinian Highland has two genera: *Stenocephalomys*, a water rat, and *Desmomys*, one of the African meadow mice. *Xerus*, a ground squirrel, *Ammodillus* and *Microdillus*, gerbils, *Heterocephalus*, the naked mole-rat, and *Pectinator*, a large rock rat, are confined to the Somali Arid District. *Geosciurus*, a ground squirrel, *Gliriscus* and *Graphiurus*, dormice, *Petromyscus*, a rock mouse, *Bathyergus*, the sand mole-rat, and *Petromys*, the dassie-rat, are found only in the South West Arid District.

There is little evidence in favor of a South African Subregion. All genera peculiar to South Africa have related genera elsewhere in the East African Subregion or throughout

Africa, except possibly *Petromys,* which is not certainly a ctenodactylid.

In general the Ethiopian fauna is relatively distinct from that of all other regions. Apparently the closest affinity is with the Oriental Region, especially if the paleontological evidence is considered. In all probability the hystricomorphs of Africa are independently derived from primitive northern hystricomorph rodents, and resemblances with the South American types are parallel rather than indicative of close relationship.

SECTION OF PSYCHOLOGY

March 20, 1939

Doctor George K. Bennett, Psychological Corporation of New York: *Psychological Factors in Secretarial Success.*

A survey of previous research in the prediction of capacity to learn shorthand and typewriting reveals no basis for the selection of potentially successful students in this field. Both commercial high schools and private secretarial schools admit large proportions of inept pupils. The incidence of failure is in the vicinity of forty per cent among those schools requiring reasonable commercial competence for graduation. Under these circumstances, any method that will discriminate between the better and worse candidates for this type of training should have significance for both the individual candidate and society.

This investigation of secretarial aptitude was begun in 1932. The first battery of tests included standard measures of intelligence, personality, English grammar and a specially constructed miniature test of typewriting. This battery was used in a private school in New Haven. The criterion was typewriting speed. None of the paper and pencil tests yielded a significant correlation, but the miniature test correlated above .6. Complete scores were available for only forty-seven subjects.

The following year the experiment was repeated with some changes in the same school. Shorthand and accounting proficiency were added as criteria. The American Council on Education Psychological Examination was used as an intelligence test in the hope that the artificial language section would serve as a predictor of shorthand aptitude. The miniature typewriting test was retained. This investigation started with ninety-eight subjects in the fall and ended with only twenty-four the following May. Some students failed and others left school for sundry reasons. Many of those who remained

dropped one or more of the three major courses. So far as any conclusions could be drawn from the results of so few subjects the miniature test again predicted typewriting capacity and none of the tests predicted shorthand. Accounting grades appeared to have some relationship to arithmetic test scores.

In the fall of 1934 a third attempt at this type of prediction was made. One hundred twenty-six girls in New Haven Commercial High School were tested during the first weeks of training in stenography and typewriting by means of the miniature typewriting test, the Otis Group Intelligence Test and an experimental shorthand aptitude test. This measure was intended to approach the shorthand process by requiring the subject to translate digits into essentially meaningless symbols and subsequently to transcribe, by hand, these symbols into digits. Criterion scores, obtained the following spring, consisted of typewriting speed and shorthand vocabulary knowledge. Multiple correlations were in the vicinity of .3 for both typewriting and shorthand. Low positive correlations were obtained between the intelligence scores and both criteria. The new shorthand test appeared to predict both shorthand and typewriting better than did the cumbersome miniature typewriting test.

This study produced negligible positive results, although it demonstrated the futility of attempting this type of research without the full cooperation of the school authorities and a much larger number of subjects.

In 1937 such an opportunity presented itself. A large private secretarial school of high standards became concerned with the high incidence of failure among its students and permitted us to test the entire entering class of six hundred. This class is divided almost equally between high school graduates qualified for college entrance and students who have completed one or more years of college. Two tests were used with these subjects. The first was a group test modification of the analogous shorthand test previously used. The second was a test of recognition and correction of misspelled words occurring in business correspondence. The miniature typewriting test was

discontinued because it was unsuited for group use. An intelligence test appeared unnecessary in view of the rigid educational requirements.

Success or failure at the end of the first year of instruction was used as a criterion. Seventy per cent attained sufficient competence to be awarded a certificate and thirty per cent did not. This measure of success is more arbitrary than a speed criterion but it offers the compensating advantages of complete objectivity and appreciable practical value.

Complete data for 505 girls were obtained. Of these 211 had previously had some instruction in either typewriting or shorthand. These were treated as one group and the remaining inexperienced girls as a second group. Correlations between test scores and success were almost exactly equal for both groups. The spelling score correlated .48 and the shorthand score .27. The multiple correlation was .54 for each group. Students with previous stenographic training scored no higher on the tests, but had an increased incidence of success. This yields a tetrachoric correlation of .2 and increases total prediction to approximately .6. Because of the large number of subjects and small number of variables the multiple correlation has a small standard error and can be relied upon at least for that school.

Although a correlation of .6 is not highly efficient from a theoretical standpoint, it may have good practical value. Establishing regression equations to estimate probability of success and applying these to the five hundred cases we obtained the following results: Of those predicted as having

25% or less probability of success —17% succeeded
25% to 50% probability of success —45% succeeded
50% to 75% probability of success —68% succeeded
75% or greater probability of success—89% succeeded

This school has facilities for about 80 per cent of its applicants. Since students are accepted in order of application if they possess the required education it is reasonable to assume

equal aptitude for the entire group. If the results of these tests are used to eliminate the poorest fifth of the applicants, 80 per cent of those admitted may be expected to acquire satisfactory skill.

This is not a completed study. We plan to add to these two tests others of English, vocabulary and intelligence in the hope that some of these may contribute to prediction.

NEW MEMBERS

Elected March 6, 1939

SUSTAINING MEMBERS

Altamura, Mario R., Organic Chemist, Woodbury, N. J.

Becket, Frederick M., D.Sc., LL.D., President, Union Carbide & Carbon Research Laboratories, Inc., New York, N. Y.

Malcolm, Wilbur G., Ph.D., Executive Director, Lederle Laboratories, Inc., Pearl River, N. Y.

ACTIVE MEMBERS

Abramson, Harold Alexander, M.D., Assistant Professor, Physiology, Columbia University, New York, N. Y.

Alexander, Jerome, M.S., Consulting Chemist and Engineer, New York, N. Y.

Bauer, Johannes Henrik, M.D., Director, Laboratories of the International Health Division, Rockefeller Foundation, New York, N. Y.

Blain, Alexander W., M.D., Attending Surgeon, Alexander Blain Hospital, Detroit, Mich.

Blanchard, Kenneth C., Ph.D., Associate Professor, Biology, New York University, New York, N. Y.

Boyden, Alan A., Ph.D., Associate Professor, Zoology, Rutgers University, New Brunswick, N. J.

Brill, Norman Q., M.D., Member, Neuropsychiatric Staffs, Montefiore, Morrisania, Post Graduate and Sydenham Hospitals, New York, N. Y.

Condit, D. Dale, M.A., Geologist, Standard Vacuum Oil Co., New York, N. Y.

Conway, Catherine, Psychologist, New York, N. Y.

Cowley, William H., Ph.D., LL.D., President, Hamilton College, Clinton, N. Y.

Halpern, Joseph B., B.S., Tutor, Geology, Brooklyn College, Brooklyn, N. Y.

Heidelberger, Michael, Ph.D., Chemist to Medical Service, Presbyterian Hospital; Associate Member, Rockefeller Institute, New York, N. Y.

Macht, Maurice L., B.S., Chemist, Supervisor Research, Plastics Division, E. I. duPont De Nemours & Co., Arlington, N. J.

Martin, Walton, M.D., Clinical Professor, Surgery, Columbia University, New York, N. Y.

Paige, Sidney, Senior Geologist, N. Atlantic Division, U. S. Army Engineers, New York, N. Y.

Pease, Marshall Carleton, M.D., Clinical Professor, Pediatrics, N. Y. Post Graduate Medical School and Hospital, Columbia University, New York, N. Y.

Pickels, Edward G., Ph.D., Staff Member, International Health Division, Rockefeller Foundation, New York, N. Y.

Reeside, John B., Jr., Geologist in Charge, Paleontology and Stratigraphy, U. S. Geological Survey, Washington, D. C.

Schubert, Maxwell P., Ph.D., Chemist, Rockefeller Institute, New York, N. Y.

Simpson, Ray H., Ph.D., Instructor, Psychology, Barnard College, New York, N. Y.

Smyth, Charles Phelps, Ph.D., Professor, Chemistry, Princeton University, Princeton, N. J.

Weinstock, Harry I., M.D., Psychiatrist, Beth Israel Hospital, New York, N. Y.

Zahnd, Hugo, Ph.D., Assistant Professor, Chemistry, Brooklyn College, Brooklyn, N. Y.

ASSOCIATE MEMBERS

Beard, Howard H., Ph.D., Professor, Biochemistry, Louisiana State University, School of Medicine, New Orleans, La.

Bernheim, Frederick, Ph.D., Associate Professor, Physiology and Pharmacology, Duke University, School of Medicine, Durham, N. C.

Bertucci, Emile Augustus, M.D., Medical Examiner, Orleans Parish School System, New Orleans, La.

Bethea, Oscar W., M.Phar., Professor, Clinical Medicine, Tulane University, New Orleans, La.

Bittner, John J., Ph.D., Research Associate, Jackson Memorial Laboratory, Bar Harbor, Maine.

Blayney, James Roy, D.D.S., Director, Zoller Memorial Dental Clinic, University of Chicago, Chicago, Illinois.

Boyden, James R., Norwalk, Calif.

Brewer, N. R., Department Fellow, Physiology, University of Chicago, Chicago, Ill.

Bridwell, John Colburn, B.S., Entomologist, U. S. National Museum, Washington, D. C.

Brougher, John C., M.D., Instructor, Surgery, Medical School, University of Oregon, Eugene, Oregon.

Brown, Thomas R., M.D., Associate Professor, Medicine, Johns Hopkins University, Baltimore, Md.

Brown, V. E., Ph.D., Paleobotanist, Devereux, Ga.

Day, Harold M., B.S., Research Chemist, Cos Cob, Conn.

Dedell, Thomas R., B.S., Graduate Student, Physical Chemistry, Yale University, New Haven, Conn.

Devney, Dorothy Claris, M.A., Instructor, Zoology, College of St. Scholastica, Duluth, Minn.

Johnston, John C., B.A., Graduate Student, Geology, University of Wyoming, Laramie, Wyoming.

Knox, Arthur S., Instructor, Geology and Paleobotany, Tufts College, Medford, Mass.

Kindwall, J. A., M.D., Staff Member, Clifton Springs Sanitarium, Lochland School, Geneva, N. Y.

Lauffer, Max A., Jr., Ph.D., Assistant, Physical Chemistry, Rockefeller Institute, Princeton, N. J.

Leonardon, E. G., Geologist, Schlumberger Well Surveying Corporation, Houston, Texas.

Lochman, Christina, Ph.D., Instructor, Geology, Mt. Holyoke College, South Hadley, Mass.

McCollum, Leonhard F., Geologist, Carter Oil Company, Tulsa, Okla.

Marble, John Putnam, Ph.D., V. Chairman, Research Council, Division Geology and Geography, Washington, D. C.

Olmsted, Elizabeth Warren, Assistant, Geology, Smith College, Northampton, Mass.

Ploger, Louis W., Assistant Professor, Geology Department, Syracuse University, Syracuse, N. Y.

Postel, A. Williams, Lecturer, Geology and Mineralogy, University of Pennsylvania, Philadelphia, Pa.

Price, Llewellyn J., Museum of Comparative Zoology, Cambridge, Mass.

Rider, Charles R., Geologist, Drilling and Exploration Co., Amarillo, Texas.

Rogers, Cleaves L., A.B., Graduate Student, Geology, Princeton University, Princeton, N. J.

Speed, Carleton D., Jr., B.S., President, Speed Oil Company, Houston, Texas.

Young, Walter Jorgenson, Ph.D., Head Department, Philosophy and Psychology, Mary Washington College, Fredericksburg, Va.

Zuck, Theodore Thomas, M.D., Acting Professor, Anatomy, Western Reserve University, Cleveland, Ohio.

STUDENT MEMBER

Marion, Stephen P., A.M., Teacher, Brooklyn College, Brooklyn, N. Y.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 May, 1939 No. 7

SECTION OF GEOLOGY AND MINERALOGY

April 3, 1939

Doctor Joe Peoples, Wesleyan University, Middletown, Conn.: *The Stillwater Igneous Complex, Montana.* (This lecture was illustrated by lantern slides.)

A banded igneous complex of norite, gabbro, pyroxenite, peridotite, and related rocks is exposed for thirty miles along the northeastern margin of the Beartooth Range in Montana. This series of igneous rocks which has been called the Stillwater Complex from its magnificent exposure in the canyon of the Stillwater River is adjoined on the north by Paleozoic sedimentary rocks and on the south by pre-Cambrian granite and metamorphosed sediments. Evidence is presented that the igneous complex is a sheet or lopolith intrusive into pre-Cambrian sedimentary rocks and in turn intruded by pre-Cambrian granite; that the top of the complex was eroded before the deposition of the Middle Cambrian which rests unconformably on it; that during the Laramide orogeny the Stillwater Complex and overlying Paleozoic beds were tilted, and, in part of the area, turned on edge and thrust to the northeast along the Beartooth thrust fault.

TRANSACTIONS of the New York Academy of Sciences, Series II, Volume 1, No. 7, May, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

The Stillwater Complex consists of a number of rock types made up of a relatively few minerals which vary greatly in their relative proportions. The important primary minerals are plagioclase (labradorite and bytownite), diopside, orthopyroxene, olivine, and chromite. The arrangement of the rock types in stratiform layers is one of the most striking features of the complex. Within some of the layers, the platy minerals such as pyroxene and plagioclase are arranged with their two long axes in the plane of banding. This is a planar structure; no indication of linear structure was found.

The complex has been divided into four zones: (1) the Basal Zone of norite considered to be the chilled primary magma; (2) the Ultramafic Zone consisting of pyroxenite, harzburgite, and dunite; (3) the Banded Zone containing norite, anorthosite, anorthositic norite, and troctolite in stratiform layers; and (4) the Upper Zone of anorthosite, gabbro, norite, and related rocks.

Several columnar sections of the complex are compared and the various zones show the following thicknesses:

Upper Zone	800 to 6000 feet
Banded Zone	3700 to 7350 feet
Ultramafic Zone	1850 to 3300 feet
Basal Zone	0 to 200 feet

The East Boulder Plateau section of the complex has a thickness of over 16,000 feet.

The mineral deposits associated with the complex consist of chromite in the Ultramafic Zone, nickel-copper sulfides of the Sudbury type at the base in several places, and disseminated platinum at several horizons in the Banded and Upper Zones.

The Stillwater Complex shows remarkable similarity to the Bushveld Complex in rock types, mineral variations, primary structure, and associated mineral deposits. Reasons for believing in similarity of magma type, mode of intrusion, and origin of rock types are presented. The Stillwater Complex is also compared with other lopoliths such as the Duluth gabbro, Minnesota; the Sierra Leone lopolith, West Africa; the Bay of Islands lopolith, Newfoundland; and others.

The origin of the various rock types of the Stillwater Complex is believed to be the result of the differentiation in a lopolithic chamber of a magma approximating the composition of the basal norite. Possibly additions of magma were made as differentiation progressed. Movement of crystals under gravity is believed to have been the most important factor in development of the various rock types, but additional factors are needed to explain the banding and an intrusive dunite in the Ultramafic Zone.

SECTION OF BIOLOGY

Joint Meeting with the Section of Physics and Chemistry
April 10, 1939

C. W. Coates, New York Aquarium, and Richard T. Cox, New York University: *Observations on the Electrical Discharge of the Electric Eel.* (This lecture was illustrated by lantern slides and demonstration with the oscillograph.)

The electric eel, *Electrophorus electricus* (Linnaeus), is the largest of a number of electric fishes, none of which is related to each other. It has also the greatest electric power of any of these fishes.

It is native to the fresh waters of northern South America, ranging from the Amazon basin to the Orinoco basin. The largest recorded specimen exceeded eight feet in length and weighed sixty-six pounds.

The head and body of the fish do not exceed one-fifth of the total length and weight, the remaining four-fifths being tail, of which about two-thirds is electric tissue. The electric tissue is divided into three pairs of electric organs, the largest beginning immediately at the end of the body and running on either side with a nearly uniform cross-section to a point about equidistant between snout and end of tail where it begins to taper off. These organs are called the *large electric organs.* A second pair of organs start where the large electric organs begin to taper, lying about the large electric organs and enlarging at the expense of the first pair. These organs are called the *bundles of Sachs.* The third pair of organs is buried in the tissue at the base of the anal fin and reaches from the end of the body to the end of the tail. This pair is small in cross-section and is called the *organs of Hunter.*

The skin is naked and of somewhat complicated structure. It is from thirty to forty cells deep, and is interlaced with a number of cone-shaped prolongations of the corium extending at right angles to the surface of the body. These prolongations are tipped with palisade-like cells.

The discharge can be made at will, but the control of its intensity by the fish is restricted. There is a characteristic discharge of the bundles of Sachs which is always the same and which is known as the *minor discharge.* This organ can be discharged without the others. The electromotive force is of the order of forty volts. This discharge is given frequently while the fish is swimming. The large electric organs have also their characteristic discharge, known as the *major discharge.* Its electromotive force may attain five hundred volts in a mature eel. This is liberated in trains of from two to five discharges, following a minor discharge. The duration of a single discharge of either type is of the order of two one-thousandths of a second, and the common interval between successive major discharges in a train is of the order of a one-hundredth of a second.

There is sometimes observed a discharge of intermediate and irregular voltage occurring between a minor discharge and the following train of majors. The origin of this discharge is not altogether certain, but its power indicates that it must be a discharge of the large organs with some modification, perhaps produced by the simultaneous discharge of the organs of Hunter.

The major discharge is used to defend the eel against enemies and to immobilize its prey, which appears to be smaller fishes only.

Although the electric eels suffer no injury from other eels, they can detect the discharge of other eels at some distance and are attracted to the discharging fish, particularly to the anterior end which is always electrically positive.

The major discharge traverses the large electric organ as a pulse of potential gradient running from anterior to posterior with a speed of the order of a thousand meters a second. The discharge at each part of the organ is "triggered" by the nerve running from the spinal cord to that part. The fact that the speed of propagation is much higher than the speed observed in nerves suggests that the fish has a mechanism for

retarding the discharge of the anterior part until the pulse reaches the posterior part.

The discharge of the large electric organs through various electric circuits has been studied in some detail and analysis of the measurements made shows that the electric tissue of the organ can be schematically represented as a constant electromotive force and a variable internal resistance. The discharge is produced by the sudden lowering of this resistance from its high value when the organ is at rest to a value low enough to produce a current around one ampere on short circuit.

By gentle prodding, the eel can be stimulated into releasing a thousand discharges in about three minutes. The fatigue produced by these discharges shows itself by an increase, which may be as high as eight-fold, in the internal resistance of the tissue during discharge. This evidence of fatigue is almost the same whether the eel discharges on open circuit or through a low resistance, and seems, therefore, not to depend on the total quantity of electricity passed. Recuperation from this fatigue, as measured by the resistance, is almost complete within a few minutes after the stimulation has ceased. The tissue is rectifying, as shown by the fact that a battery of higher electromotive force than that of the organ, connected so as to oppose the discharge, does not reverse the direction of the current.

SECTION OF PSYCHOLOGY

April 17, 1939

Professor Harry L. Hollingworth, Columbia University: *Psycho-Dynamics of Chewing.*

Motor automatisms, collateral and incidental to some main occupation, are characteristic of the behavior of men and of the lower animals. Stamping, swaying, tapping, biting, chewing, are random examples. These are often said to be related to tension, either as signs of it or by way of expression or relief. Little experimental work has been done, and the present paper summarizes a detailed investigation, by experimental methods, on groups of human subjects, using automatic chewing as the automatism investigated. Control conditions, to match the use of the conventional masticatory, were provided by the use of mint wafers, giving the same sensory stimulation but unaccompanied by the motor activity. The results of the whole series emerge in a well articulated total pattern.

Study of the energy cost of sustained chewing, as reflected in pulse rate and in metabolism measures showed that this cost varied with the motor activity level in general. The more active the general level, the lower the added costs of the motor automatism. With vigorous activity the motor automatism is carried with no added energy cost, due to the reduction of random and restless movement which accompanied sustained mastication.

Muscular tension throughout the body is measurably reduced while chewing. This was shown in a variety of ways. Direct measures of tension in an unoccupied set of muscles, during active work, showed this reduction. Objective ratings, by trained observers, of the frequency of random restless movements showed these to decline in periods when the masticatory was in use. Subjective ratings, by the workers, of their felt strain and relaxation, during work periods, showed decreased feelings of strain and a general posture of relaxation during automatic chewing.

The rate of fluctuation of visual designs seen in equivocal perspective was found to increase. This is the opposite of the alleged effect of fatigue on this index, and more closely resembles the effect of tea, coffee, and similar "refreshing" agents, as reported in the literature.

Measurement of the energy put into the working muscles of the main occupation, while chewing, showed a distinct and measurable increase. Writing pressure (point pressure) increases while chewing is engaged in, and in a specially designed pin target test, arm thrusts were made more vigorously during the periods of automatic chewing. The motor automatism is thus "dynamogenic," in the sense that, by acting as an overflow for motor restlessness it conserves energy. The surplus thus saved may be unwittingly discharged into the channels engaged with the main occupation.

Speed in typing was found to increase while chewing. With one subject this increase was at the expense of accuracy. The other typist made notable speed gain with correlated improvement in accuracy. In spite of this increased speed, both typists reported to have proceeded with reduced tension. There are apparently individual idiosyncrasies here as elsewhere, and sustained mastication does not affect all workers in the same way.

Marked individual differences were found in the characteristic muscular tension with which individuals worked. The workers on a higher level of tension, in general, experienced the greater relaxation when the motor automatism was introduced. Different degrees of tension were also found to be consistently characteristic of different types of intellectual and motor work.

Out-put studies were made on nine different kinds of work. No evidence was found that sustained chewing (controlled for sensory stimulation and like factors) interfered with out-put. There was some indication of an adverse effect on work involving new learning, with facilitation in the case of simple repetitive activities, but the data are inadequate on this point.

The interpretation of these results offered is a simple one. It involves no specific neurological hypotheses, although the results may also be explained by various current theories of neuro-muscular dynamics. The primary role of chewing is in the mastication of food. Eating is ordinarily a quiet and relaxing occupation, with random restlessness at a low point. While eating we sit; we relax; and the general feeling tone is one of agreeableness and ease.

An important item of the eating situation is chewing. We suggest that, as a result of this contextual inclusion, chewing, as a "conditioned stimulus," brings with it, whenever it is sustained, a posture of relaxation.

Chewing, in other words, serves as a reduced cue, and to some degree redintegrates the relaxation of meal time. The remaining parts of the picture follow from this redintegrated posture of relaxation. The motor automatism, then, does provide relief from tension, and the tension that is thus relieved is muscular. As the result of this tension reduction, on an activity level higher than that of a basal resting state, the motor automatism is carried at little or no energy cost, and surplus energy is rendered available for the main occupation. The same or improved out-put is achieved with a subjective relaxation that is also objectively identifiable.

SECTION OF ANTHROPOLOGY

APRIL 24, 1939

PROFESSOR RALPH LINTON, Columbia University: *Culture Sequences in Madagascar.*

The great island of Madagascar presents a puzzle to ethnologists. In spite of its close proximity to the African mainland its inhabitants all speak languages of the Malayo-Polynesian stock and possess many elements of Indonesian culture. Physically the population shows a mixture of Negroid and Indonesian characteristics but with considerable local differentiation. Predominantly Negroid groups occupy the coastal lowlands; predominantly Indonesian ones the high interior plateau. It has usually been assumed that the aboriginal population was made up of Negroes who were conquered and partially displaced by a relatively late invasion of Malays but native traditions and the geographic distribution of cultures and physical types indicates that this is, at best, an over-simplification.

Tribes living in the dense rain forests of the eastern slope of the island give evidence in both their traditions and physical type of the presence here of Negritos who, both physically and culturally, resembled those of Indonesia rather than Africa. These Negritos may have been the true aborigines.

At a later time the island seems to have been invaded and largely occupied by a brown-skinned, wavy-haired group with a rather simple culture reminiscent of that of the more primitive tribes of Indonesia. These people were ignorant of weaving, dressing in mats and bark-cloth, made no pottery and had neither the shield nor the bow. However, they practiced agriculture, including the raising of taro, yams and unirrigated rice, and had some knowledge of iron working. These people unquestionably were of Indonesian origin, but their culture suggests that they left that region before it was affected by Hindu civilization.

A second wave of Indonesian migrants appear to have come to Madagascar from Sumatra shortly after the beginning of the Christian era. These people brought with them such culture elements as warp dyed weaving and irrigated rice culture. According to the traditions of their descendants they landed first on the southwest coast of the island, but rapidly forced their way into the central plateau, where they established themselves as an aristocracy among the older Indonesian population.

The Negroid tribes of Madagascar do not agree in their physical type with the inhabitants of the adjoining mainland. They resemble much more closely the peoples of far northeastern Africa. It is impossible to say whether their ancestors arrived before or after the Sumatran immigrants but they certainly arrived after the people of simple Indonesian culture and expelled them from the lowlands. In this expulsion they may have been aided by malaria, for the Indonesian population is highly susceptible to this disease while the Negroids are resistant to it. At the beginning of the historic period the Indonesians occupied the malaria-free parts of the island, the Negroids the malarial parts. This condition is closely parallel to that in Melanesia where the occasional groups of brown-skinned, wavy-haired people are limited to small malaria-free islands.

The route by which the various Indonesian settlers reached Madagascar must remain problematical for the present but it seems fairly certain that they did not sail directly across the Indian Ocean. If they had done so they would almost certainly have discovered and colonized various islands there which were unoccupied when first visited by Europeans. It seems more probable that they crossed well to the north and reached Madagascar by following the African coast southward. If such was the case, the African natives must have been subject to considerable early contact with Indonesian culture and this possibility should be considered in all attempts to reconstruct African origins.

NEW MEMBERS

Elected April 3, 1939

SUSTAINING MEMBERS

Clarke, Hans T., D.Sc., Professor, Biological Chemistry, Columbia University, New York, N. Y.

Peck, Samuel M., M.D., New York, N. Y.

Zellers, John A., Vice-President, Remington Rand, Inc., New York, N. Y.

ACTIVE MEMBERS

Allen, Alexander Aloysius, M.A., D.D.S., Instructor, Chemistry, New York University, New York, N. Y.

Allen, Willard Myron, M.D., Strong Memorial Hospital, Rochester, N. Y.

Becker, Marie F., New York, N. Y.

Block, Richard J., Ph.D., Research Chemist, New York State Psychiatric Institute and Hospital, New York, N. Y.

Boyers, Luther M., Berkeley, Calif.

Butler, Rear Admiral Charles St. John, M.D., U. S. Navy Dept., Washington, D. C.

Chesire, Leone E., S. M., Statistician, Psychology, Columbia University, New York, N. Y.

Craig, Lyman C., Ph.D., Chemist, Rockefeller Institute, New York, N. Y.

Davidheiser, Lee Y., Ph.D., Head, Chemistry Dept., Wagner College, Staten Island, N. Y.

Davidson, David, Ph.D., Assistant Professor, Chemistry, Brooklyn College, Brooklyn, N. Y.

Dawson, J. A., Ph.D., Associate Professor, Biology, College of the City of New York, N. Y.

de Barenne, J. G. Dusser, M.D., M.A., Sterling Professor, Physiology, Yale University, New Haven, Conn.

Duschak, Klara A., M.D., New York, N. Y.

Gundersen, Alfred, Dr. de l'Univ., Paris, Brooklyn Botanic Garden, Brooklyn, N. Y.

Hotchkiss, Rollin D., Ph.D., Assistant, Rockefeller Institute, New York, N. Y.

Kaplan, Emanuel B., M.D., Adjunct Orthopædic Surgeon, Hospital for Joint Diseases, New York, N. Y.

Lehnerts, Edward M., A.M., New York, N. Y.

Lippmann, Robert, M.D., New York, N. Y.

Lough, Orpha M., Ph.D., Principal, The Mills School of Kindergarten-Primary Teachers, New York, N. Y.

Lufkin, H. D., Executive, Westinghouse Elec. & Mfg. Co., New York, N. Y.

MacFayden, Douglas A., M.D., Assistant, Hospital, Rockefeller Institute, New York, N. Y.

Marlies, Charles A., Chem.E., Ph.D., Assistant Professor, Chemical Engineering, College of the City of New York, N. Y.

Meister, Morris, Ph.D., Principal, High School of Science, Bronx, New York, N. Y.

Mooney, Melvin, Ph.D., Physicist, U. S. Rubber Co., Passaic, N. J.

Paige, Beryl H., M.D., Assistant Professor, Pathology, Columbia University, New York, N. Y.

Quam, G. Norman, Ph.D., Associate Professor, Chemistry, Long Island University, Brooklyn, N. Y.

Reimer, Marie, Ph.D., Professor, Chemistry, Barnard College, New York, N. Y.

Renzetti, Nicholas A., A.M., Assistant, Physics, Columbia University, New York, N. Y.

Rothen, Alexandre, Chem.E., Sc.D., Associate, Physical Chemistry, Rockefeller Institute, New York, N. Y.

Sagal, Zachary, M.D., New York, N. Y.

Schuyler, Virginia, M.S., Assistant, Psychology, Teachers College, Columbia University, New York, N. Y.

Wimmer, Curt P., Phar.D., Ph.M., Professor, Pharmacy, Columbia University, New York, N. Y.

Woldman, Norman E., Ph.D., Chief Metallurgical Engineer, Bendix Aviation Corp., East Orange, N. J.

Zinsser, Hans, M.D., Sc.D., Professor, Bacteriology, Harvard University Medical School, Boston, Mass.

ASSOCIATE MEMBERS

Anderson, Rudolph J., Ph.D., Professor, Chemistry, Sterling Chemistry Laboratory, Yale University, New Haven, Conn.

Apfelbach, Carl W., M.D., Attending Pathologist, Cook County Hospital, Chicago, Ill.

Balls, Arnold K., Ph.D., Principal Chemist, U. S. Dept. of Agriculture, Food Research Division, Washington, D. C.

Beardslee, Henry C., A.M., Perry, Ohio.

Bennett, Henry W. N., M.D., Dermatologist, Elliot Hospital. Manchester, N. H.

Bennighof, Cloyd L., M.S., Assistant Professor, Biology, Western Maryland College, Westminster, Maryland.

Bickel, Beatrix A., M.D., Chief Assistant, Army Medical Library, Washington. D. C.

Birge, Edward A., Ph.D., Sc.D., LL.D., President Emeritus, University of Wisconsin, Madison, Wisconsin.

Bishop, Louis Bennett, M.D., formerly Head, Dept. of Pediatrics, Yale Medical School; 450 Bradford St., Pasadena, Calif.

Black, Ralph W., A.M., formerly Psychologist, United States Service, 102 Rollstone St., Fitchburg, Mass.

Blincoe, Homer R., M.D., Professor, Gross Anatomy, Emory University School of Medicine, Emory University, Ga.

Bock, Joseph C., Ph.D., Director, Dept. of Chemistry, Marquette University School of Medicine, Milwaukee, Wisconsin.

Bond, William R., M.D., Medical Research, Schering Corp., Bloomfield, N. J.

Bowles, Oliver, Ph.D., Asst. Chief Engineer, Nonmetal Economics Division, U. S. Bureau of Mines, Washington, D. C.

Brown, Alice L., Ph.D., Research Fellow, Zoology, Washington, D. C.

Brown, Clarence F. G., M.D., Chicago, Ill.

Buchanan, J. William, Ph.D., Professor, Zoology, Northwestern University, Evanston, Ill.

Bruner, Henry L., Sc.D., Emeritus Professor, Zoology, Butler University, Indianapolis, Ind.

Calvery, Herbert O., Ph.D., Princ. Pharmacologist and Chief, Div. of Pharmacology, U. S. Dept. of Agriculture, Washington, D. C.

Chedsey, William R., E. M., Director, Missouri School of Mines, Rolla, Mo.

Cushing, Harvey, Sc.D., LL.D., Professor Emeritus, Neurology, Yale University School of Medicine, New Haven, Conn.

Karapetoff, Vladimir, M.M.E., Sc.D., D.Mus., Electrical Engineer, Cornell University, Ithaca, N. Y.

McCarter, W. Blair, Geologist, Tide Water Oil Company, Lafayette, La.

Millison, Clark, Geologist, Tulsa, Oklahoma.

Newland, David H., Sc.D., State Geologist of New York, Albany, N. Y.

Olcott, David P., Geologist, Humble Oil & Rfg. Co., Houston, Texas.

Prettyman, T. M., Geologist, Fort Worth, Texas.

Prindle, Louis M., A.B., formerly U. S. Geological Survey, 104 East Melrose St., Chevy Chase, Maryland.

Roberts, Dwight C., Geologist, Los Angeles, Calif.

Whitaker, Douglas M., Ph.D., Professor, Biology, Stanford University, Calif.

White, Abraham, Ph.D., Assistant Professor, Physiological Chemistry, Yale University School of Medicine, New Haven, Conn.

Wilbur, Gertrude H., M.D., Resident Physician, Brandywine Sanatorium, Marshallton, Delaware.

Wiley, Lester E., Ph.D., Assistant Professor, Psychology, Ohio Wesleyan University, Delaware, Ohio.

Williams, John W., M.D., Associate Professor, Biology, Massachusetts Institute of Technology, Cambridge, Mass.

Woods, J. P., Ph.D., Geophysicist, Shell Petroleum Corporation, Houston, Texas.

Woodward, Alvalyn, Ph.D., Assistant Professor, Zoology, University of Michigan, Ann Arbor, Mich.

Woodworth, Robert H., Ph.D., Chairman, Division of Science, Bennington College, Bennington, Vt.

Wylie, Margaret, Ph.D., Professor, Psychology, Cornell University, Ithaca, N. Y.

Yamanouchi, Shigeo, Ph.D., Sc.D., Professor, Botany, University of Chicago, Chicago, Ill.

Yanovsky, Elias, Ph.D., Chemist, U. S. Dept. of Agriculture, Bureau of Chemistry and Soils, Washington, D. C.

Yeager, J. Franklin, Ph.D., Senior Entomologist, Bureau of Entomology & Plant Quarantine, Beltsville, Md.

Young, Walter J., Ph.D., Head, Dept. of Philosophy and Psychology, Mary Washington College, Fredericksburg, Va.

Zufall, Chalmers J., Ph.D., Professor, Materia Medica, Purdue University School of Pharmacy, Lafayette, Ind.

STUDENT MEMBERS

Schoenborn, Henry W., A.B., Graduate Assistant, Protozoology, New York University, New York, N. Y.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 1 JUNE, 1939 No. 8

SECTION OF GEOLOGY AND MINERALOGY

MAY 1, 1939

PROFESSOR FREDERICK K. MORRIS, Massachusetts Institute of Technology: *The Pacific Ocean, Real and Maligned.* (This lecture was illustrated by lantern slides.)

The Pacific is by far the biggest of the earth's oceans. Its area is 2.3 times that of the Atlantic and 2.5 times that of the Indian Ocean. It is more consistently deep than any other; its floor, almost everywhere that tests have been made, yields positive anomalies indicating a floor of heavy rock, like basalt. Earthquake vibrations move faster under its floor than through any other equally shallow portion of the crust. The Pacific is bordered, especially on the west, by arc-shaped chains of mountains; and all students agree that the arc-structures extend far seaward, as in the Mariana and Bonine chains. On the American side a great contrast is observed; for none of the arcs lie out in the ocean; and not all the shore-line ranges are composed or structured like the Asiatic arcs; although all belong to the same mountain-making history. An active earthquake-belt coincides with the entire island-arc system, and the Pacific shores of the Asiatic continent itself are everywhere subject to earthquakes of less intensity than in the arcs. In contrast, the broad

TRANSACTIONS of the New York Academy of Sciences, Series II, Volume 1, No. 8, June, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

basins of the open ocean are almost free from strong shocks. Much less continuous than the earthquake belt is the girdle of volcanoes around the Pacific—often called the Ring of Fire. It is a striking feature, especially when contrasted with the paucity of volcanoes in other oceans and along their shores.

Out in the bowl of the Pacific three areas may be distinguished: a northern and a southern basin, each almost wholly devoid of islands; and a central belt studded with archipelagoes. These islands are all volcanoes whose lavas are generally though not exclusively basaltic. But westward toward Australia, continental islands are found; and here, according to Gutenberg, the Pacific floor behaves more like a continent than does any other portion of the vast basin.

These are elementary facts and widely published. What interpretations are offered for these facts, and how well do the inferences bear scrutiny? Osmund Fisher suggested, in 1876, that the Pacific basin is the birthplace of the moon; and William Pickering, in 1910, forcefully declared that there's no other place for the moon to come from. Many, perhaps most, geologists accept it; and Arthur Holmes offering the Lowell lectures in 1936 proclaimed it as undisputed truth; showing in a series of diagrams the birth of the moon, and the modern shapes of Asia and America rimming the new-born Pacific Ocean.

The thesis holds that the departing moon tore away the earth's granite skin, leaving the bare sima exposed; that the edges of the granite shell, trying to slide out into the hole, have wrinkled the island arcs out of the basin's floor; and that around and within the amputation-scar the earth has been bleeding basalt-juice. All facts are accounted for, including the positive gravitation-anomalies, and the fast travel-time of earthquakes.

But grave objections can be brought: Bryan and Love showed that the earth has never had sufficient energy to fling out the moon centrifugally; and if it had, several small moons rather than one so disproportionately large would have been born; for in relation to the earth, the moon is the largest satellite in the Solar System. Jeffreys rescued the theory by intro-

ducing the sun as a tidal obstetrician. He showed that at best the theory is just possible; and the moon could have parted from the earth only when the earth was wholly molten. Therefore, the moon's departure would not have left a Pacific Ocean basin, for the liquid sial would have flowed over the amputation. The date of the moon's departure must have been at a substantially pre-Archæozoic time; for the oldest Archæozoic sediments tell of an earth as cool and moist as it is today. As the island arcs and their adjoining deeps were shaped during middle and late Tertiary time, the birth of the moon was too early by a billion years to account for the present configuration of the Pacific; and probably that billion needs a coefficient. The volcanoes are all of late Tertiary and Pleistocene age; so they lend even less support to the moon-Pacific theory than do the arcs and deeps. The conclusion cannot be escaped: if the moon is the earth's child at all—which is not proved—its age alone wholly acquits the moon of having dug out the Pacific basin.

But if its age were not its alibi, the moon's size would be; for if the moon were put back into the earth it would fill all the ocean basins 36 miles deep, and make them higher than any continent. The Pacific basin is far too shallow to have borne the moon.

A second interpretation is the Taylor-Wegener hypothesis which holds that the Pacific formerly contained virtually all of the earth's ocean-water and that the drifting continents have encroached upon it, narrowing the Pacific as the Atlantic widened; and moving Australia far eastward to shut off the Indian Ocean. By this theory the arcuate mountains of the Pacific are wrinkles pushed up by the advancing edges of the sliding continents. DuToit has recently presented the thesis in an attractive but unconvincing book. It's true that the puzzling glaciation of the tropical southern continents would be explained; but the hypothesis causes more difficulties than it solves. The island arcs should be more abundant along the far-moving Americas than along the nearly stable Asia; yet the reverse is true. The arcs should be made of abyssal sediments,

wrinkled up out of the deep ocean; but instead they are shallow-water types and deep-sea deposits are rare. Europe and North America, Africa and South America could easily exchange plants and animals while they were joined together; but exchange across the Pacific was impossible, for even Bering Strait was 2,000 miles wide. But during Cretaceous time while the Atlantic was narrow and the Pacific broad, western America and eastern Asia had closely related plants and animals, many of which are not found in Europe.

I am no enemy of the theory of continental drift; but must defer accepting it until supporting evidence is found of the same weight as that which supports acceptable parts of geologic science.

Quite the worst malignment of the good Pacific is the highly popular thesis that a continent inhabited by civilized men existed in the Pacific; that the ancient cultures of India, Egypt and America were derived from it. The thesis exploits the natural parellelisms between the artistries of these widely separated regions, and gilds its inconsistencies with assertions as sweeping as those of the Bible. The absence of continent and culture is due to sudden foundering that destroyed all but the remnant-crags which now form the mid-ocean archipelagoes. Were it not for the widespread popular acceptance of this thesis, I would not have reviewed it before the Academy; but I find that not only intelligent laymen but even some scientists take it seriously.

If it were true, we should find that the races in America and Asia were descended from the peoples still living in the island-remnants of the original Pacific homeland; for it is loudly claimed that the islanders are the naked survivors of the catastrophe. Instead we find them diverse, each from each; and we see clear evidence that the peoples of all three regions are Asiatic stocks, or mixtures of stocks; and that all migrated from Asia at diverse stages of a long and complex history. The islands, instead of being crags left by the rifting of a sunken continent, are volcanoes built up by successive, orderly lava-

flows until they now stand above the ocean. Their proven history as volcanic islands is far longer than their supposed history as crags; that is, they were normal oceanic islands long before the human history of the Pacific continent is postulated; and several million years before the American and Asiatic civilizations which the protagonists claim were derived from the Pacific land before its catastrophe. The statues on Easter Island are known to be Polynesian work, and give no support to the thesis of a vanished continent.

SECTION OF BIOLOGY

May 8, 1939

Doctor Carl G. Hartman, Johns Hopkins University School of Medicine: *The Physiology of Ovulation.* (This lecture was illustrated by lantern slides and motion pictures.)

1. *Physiological factors involved*

Ovulation in mammals has been attributed solely to intrafollicular pressure, which supposedly resulted in rupture of the follicle when it attained a certain degree of pressure. Sudden rise in pressure has been attributed to (a) swelling of theca interna; (b) local increase in blood pressure; (c) contraction of smooth muscle fibers of the theca externa; (d) preovulatory secretion of liquor folliculi. This "intrafollicular pressure" theory has proved inadequate, for injured follicles may become cystic yet fail to rupture despite high internal pressure. An alternative theory (Hartman in Allen's *Sex and Endocrine Glands,* 1932) views ovulation as the result of an orderly series of changes in the cellular wall of the follicle under the stimulus of the anterior pituitary gland, as has been known since Philip E. Smith's epoch-making discovery of the pituitary-gonadal relationship. After the follicle wall has thinned sufficiently, it opens at the "stigma" without violence—there are no jagged edges at the "rupture point," as had already been noted for Cryptobranchus as well as birds. In principle the new theory holds for all the vertebrate classes.

That ovulation in mammals is not a cataclysmic process was demonstrated by means of the Yale motion picture of ovulation in the rabbit shown at the end of the lecture.

The rôle of the hypophysis in ovulation has been proved in various ways, for example, by hypophysectomy and by substitution therapy. One of the most interesting chapters of the subject, however, concerns the avenue by which nerve impulses reach the anterior pituitary. To elucidate this problem the

rabbit is the subject of choice, for the rabbit female does not normally ovulate except after coitus. Parenthetically it may be stated that the ferret and the cat belong to the same behavioral type; all other mammals ovulate spontaneously. The lecturer raised the question as to whether in the other vertebrate classes one could make a similar distinction. As an example the dove and the hen were contrasted. More experimental work is needed on the lower vertebrates—an expansion of the type of researches carried on by G. Kingsley Noble and his associates at The American Museum of Natural History.

In the rabbit it has been shown that nerve impulses of coitus must reach the pituitary. This may be done, as the lecturer has, by direct electrical stimulation of the gland. Ovulation follows. It likewise follows stimulation of the central end of the cut spinal cord, but not of the distal end. Direct stimulation of the ovary has no effect. Ovulation is, therefore, hormonally conditioned. Even though the cord is cut and the cerebral hemispheres and the sympathetic chain removed, a vigorous male may yet cause ovulation in such a female through attempts at mating. Finally, after the stalk of the pituitary has been severed, with no other damage to the animal, she may copulate a great deal but will never ovulate.

Induction of ovulation in animals that remain sterile because of failure to ovulate has thus far not been found to be very successful. The lecturer related his experiences with non-ovulating monkeys and was able to report success in but few cases, by the use of gonadotropic substances. The chief difficulty lies in gauging the correct dosage: too little is ineffective, too much injures the follicles which become cystic without rupturing.

2. *The time of ovulation in the cycle*

Exact data as to time of ovulation with reference to easily recognizable signs, such as changes in the cellular content of the vagina, overt sex behavior (sexual receptivity) or the menstrual flow, has been worked out with exactitude in only a few forms. In the mouse, rat, guinea-pig and opossum ovulation is

only fairly well related to the beginning of the "cornified smear." In the guinea-pig the time relation of ovulation is more precise with reference to the time of acceptance of the female, namely 10 hours after the beginning of heat, whether coitus takes place or not. The rabbit, as is well known, ovulates 10 hours after coitus. Hence it is the classical animal for numerous lines of researches in physiology and embryology.

Of especial interest is the time of ovulation in women. Up to the present there are several tests to show that ovulation has occurred, namely: (1) endometrial biopsies; (2) analysis of pregnandiol in the urine; (3) inspection of ovaries at laparotomy. None of these make exact timing possible but offer presumptive evidence of a limited fertile period in the menstrual cycle. That the last method is the most unreliable of the three the lecturer illustrated by photographs of the monkey ovary removed at various times after accurately determined ovulations.

In the monkey, ovulation has been determined by means of (1) isolated coitus, under strict control; (2) palpation of the ovaries; (3) recovery of a hundred eggs and embryos. The monkey has a limited period of fertility. Similarly, absolutely proved data are almost non-existent for man. What few young ova have been recovered from women were all ovulated within the fertile period determined for the monkey. Not a single case of ovulation, accompanied with adequate proof, has been reported outside this period, the lecturer stated.

A motion picture of the dividing monkey ovum in tissue culture closed the lecture.

SECTION OF PSYCHOLOGY

MAY 15, 1939

LAWRENCE K. FRANK, Josiah Macy, Jr., Foundation, New York, N. Y.: *Projective Methods for the Study of Personality.*

In view of the confusion and conflicts over concepts and methods for study of personality, it is suggested that we view the emergence of personality as an outcome of the interaction of cultural agents and the individual child. In this process we see parents and teachers attempting to train the child in the prescribed social and cultural norms of action, speech and belief which he accepts with varying degrees of conformity and with feelings that permeate all his behavior. Thus the child is socialized but also individualized, so that he acts, speaks, believes and feels idiomatically.[1]

The problem of personality then is to devise procedures for revealing how the *individual* organizes and patterns experience and behavior and reacts affectively to situations idiosyncratically, as contrasted with the task of measuring his conformity to social norms through standardized tests of known validity and reliability for the age and sex *groups* to which individuals are assigned.

Recent scientific developments have shown that underlying the apparent uniformity and regularity of aggregates, individual particles, atoms, molecules and organisms are disorderly and unpredictable. Furthermore, the field concept is being used to conceptualize the part-whole relationships as a dynamic process of interacting parts that create the whole and thereby pattern their individual activities. These and other developments have shown the fallacy of reifying data into entities and the importance of studying the total unified substance or

[1] Frank, Lawrence K. The Fundamental Needs of the Child. *Mental Hygiene,* Vol. XXII, No. 3, July, 1938.

Frank, Lawrence K. Cultural Coercion and Individual Distortion. *Psychiatry,* Vol. II, No. 1, February, 1939.

organism which the older analytic methods disintegrated or destroyed.

X-rays, diffraction analysis, spectroscopic methods, polarized light, light reflections, biological assays, the Wilson Cloud Chamber, and similar procedures are being employed to discover the chemical composition and the internal structure and organization of any substance or organism and the behavior of individual particles, or parts of larger aggregates. These methods offer possibilities for revealing what is either unknowable by other means or undeterminable by the older analytic methods which disintegrated or destroyed the object under study. They indicate that it is possible to study the specific, differentiated individuality of organized structures and particulate events which are ignored or obscured by the older quantitative determinations of aggregates.

It is important to note that the reliability and the validity of these methods are determined, not by statistical procedures, but by genetic and experimental operations, that is, by using substances or structures of known composition, often made to order, so that the diffraction pattern, spectroscopic lines, etc., are established as reliable and valid indicators when found in an unknown substance or organization. The validity of these methods is also established by the concurrent use of several different procedures which yield consistent and congruous findings on the same substance or subject under study. These procedures are being increasingly used and refined because the early quantitative methods, which merely told what and how much, are too crude and inadequate to reveal not only the constituents but the spatial arrangements, as in stereochemistry. These methodological advances and the conceptions of individuality and of individuation are being accepted as investigators endeavor to reveal the range of individual differences as in the protein molecules.

The foregoing description of recent scientific developments provides a useful background and gives scientific sanction for the study of personality as a *process* of organizing experience and "structuralizing the life space" (Lewin) in a *field* which

may be conceived in terms of the individual's private world of meanings, significances, patterns and feelings, which he endeavors to impose upon all life situations. This conception offers an approach to the study of personality as a task of inducing the individual to reveal his way of organizing experience by giving him a field (objects, materials, experiences) with relatively little structure and cultural patterning, so that the personality can project upon that plastic field his idiomatic, idiosyncratic personality.[2]

Specifically, a projection method for the study of personality involves the presentation of a stimulus-situation designed or chosen because it will mean to the subject, not what the experimenter has arbitrarily decided it should mean (as in most psychological experiments using standardized stimuli in order to be "objective"), but rather whatever it means to the personality who gives it or imposes upon it his private, personal organization and needs. Projective methods for this purpose vary in materials used and in what they may elicit from subjects, as in the Rorschach ink blots, and Stern cloud pictures, in play technic, in clay, finger paints, and art expressions generally, the telling or completion of stories, in handwriting and motor adjustments, and postures, and so on. These methods may be tested for validity by the method of *temporal validation* whereby the validity of the method for an *individual,* instead of the group, is discovered by using the procedure upon the individual over a period of time sufficient to show that the personality organization thus revealed is a persistent and valid configuration for that individual. The reliability of these methods for an *individual,* not the group, may be tested by the congruity and consistency of results obtained by a variety of these procedures applied to the same individual, whereby the basic personality configuration of the individual is confirmed.

It will be seen therefore that projective methods for personality study are contrasted with the present quantitative

[2] Horowitz, Ruth, and Lois Barclay Murphy. Projective Methods in the Psychological Study of Children. *Journal of Experimental Education,* Vol. 7, No. 2, December, 1938.

procedures which attempt to measure correlations or establish factors by statistical manipulation of supposedly separate variables or traits measured on a large group of individuals at one moment in their life careers.

It may be emphasized that projective methods are not offered as a substitute for the quantitative statistical procedures but rather are designed to permit a study of the unique idiomatic individual personality which is conceived as a process of organizing experience and so must elude the investigator who relies upon methods that of necessity ignore or obscure the individual and the configural quality of his personality. Finally, it should be noted that projective methods of personality study offer possibilities for utilizing the insights into human conduct and personality expression which the prevailing quantitative procedures seem deliberately to reject.

Note: The full paper and an extended bibliography on projective methods will be published in one of the psychological journals during 1939.

THE NEW YORK ACADEMY OF SCIENCES

announces the

A. CRESSY MORRISON PRIZES FOR 1939

Two prizes of $200 each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Dinner, December, 1939, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society. These papers must embody the results of original research not previously published.

Conditions:

(1) Eligibility. Membership in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to November 1, 1939, to the Secretary of The New York Academy of Sciences at The American Museum of Natural History, Central Park West at 79th Street, New York City.

(3) Papers. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication.

It is suggested that papers submitted for the prizes be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the awards of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1939 by The New York Academy of Sciences." This statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

NEW MEMBERS

Elected May 1, 1939

ACTIVE MEMBERS

Appleby, Alfred N., M.S., Instructor, Drafting Department, College of the City of New York, N. Y.

Beardsley, Seymour W., M.A., Instructor, History, Polytechnic Institute of Brooklyn, Brooklyn, N. Y.

Bick, Edgar M., M.D., Orthopedic Surgeon, New York, N. Y.

Bing, Richard J., M.D., Instructor, Physiology, New York, N. Y.

Blau, Nathan F., Ph.D., Biochemist, Cornell University Medical College, New York, N. Y.

Bodansky, Aaron, Ph.D., Instructor, Biochemistry, Cornell University, Ithaca, N. Y.

Cannan, R. Keith, Sc.D., Dept. of Chemistry, New York University, New York, N. Y.

Cohn, Edwin J., Ph.D., Professor, Biological Chemistry, Harvard Medical School, Boston, Mass.

Daghlian, Garabed K., Ph.D., Professor, Physics, Connecticut College, New London, Conn.

Davis, May O'Connell, Westchester County Pharmaceutical Association, White Plains, N. Y.

de La Rochelle, Philippe, B. ès L., Professor, French, Directeur de l'Institut des Etudes Française, Columbia University, New York, N. Y.

Delchamps, H. J., B.E., Bell Telephone Laboratories, New York, N. Y.

Edsall, John T., M.D., Associate Professor, Biological Chemistry, Harvard Medical School, Boston, Mass.

Fishberg, Ella H., M.D., Director, Biochemical Laboratory, Beth Israel Hospital, New York, N. Y.

Foster, Goodwin L., Ph.D., Associate Professor, Biochemistry, Columbia University, New York, N. Y.

Gregersen, Magnus I., Ph.D., Professor, Physiology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Herbst, Robert M., Ph.D., Instructor, Biological Chemistry, Columbia University Medical School, New York, N. Y.

Holmes, Frank J., A.M., Instructor, Business Psychology, New York University, New York, N. Y.

Horsfall, Frank L., Jr., Ph.D., The Rockefeller Foundation, International Health Division, New York, N. Y.

Johnson, Joseph L., M.D., Ph.D., Professor, Head, Department of Physiology, Howard University School of Medicine, Washington, D. C.

Johnston, William R., Ph.D., New York, N. Y.

Karelitz, George E., M.E., Professor, Mechanical Engineering, Columbia University, New York, N. Y.

Kempf, Edward J., M.D., Wading River, Long Island, N. Y.

Killian, John A., Ph.D., Director-President, Killian Research Laboratories, Inc., New York, N. Y.

Light, Robert F., M.S., Research Chemist, The Fleischmann Laboratories, New York, N. Y.

Linton, Ralph, Ph.D., Professor, Anthropology, Columbia University, New York, N. Y.

Loewe, Siegfried, M.D., New Rochelle, N. Y.

Longsworth, Lewis G., Ph.D., Assistant, Rockefeller Institute, New York, N. Y.

Lowell, Herman H., M.A., Chemist, New York, N. Y.

McArdell, Wesley E., B.S., Teacher, Brooklyn Technical High School, Brooklyn, N. Y.

Poindexter, Charles A., M.D., Assistant Professor, Medicine, N. Y. Post Graduate Medical School, Columbia University, New York, N. Y.

Pollack, Robert T., B.S., Engineering and Research, New York, N. Y.

Powers, S. Ralph, Ph.D., Professor, Natural Sciences, Columbia University, Teachers College, New York, N. Y.

Rappaport, Israel, M.D., Associate, Medicine, Columbia Medical School, New York, N. Y.

ASSOCIATE MEMBERS

Bishop, Sherman C., Ph.D., Professor, Zoology, University of Rochester, Rochester, N. Y.

Boothby, Walter M., M.D., Professor, Experimental Metabolism, Mayo Clinic, Rochester, Minn.

Borsook, Henry, Ph.D., Professor, Biochemistry, California Institute of Technology, Pasadena, Calif.

Brassfield, Charles R., Ph.D., Assistant Professor, Physiology, University of Michigan, Ann Arbor, Mich.

Brunner, R. Maurine, Librarian, Youngstown College Library, Youngstown, Ohio.

Butt, Ferdinand Hinckley, Ph.D., Entomology, Cornell University, Ithaca, N. Y.

Colbert, Rear Admiral Leo O., C.E., Director, U. S. Coast and Geodetic Survey, Washington, D. C.

Daniels, Farrington, Ph.D., Professor, Chemistry, Wisconsin University, Madison, Wis.

Darling, George B., Dr. P. H., W. K. Kellogg Foundation, Battle Creek, Mich.

D'Autremont, Louis P., F.R.S.A., Research Chemist and Inventor, Duluth, Minn.

Dederer, Pauline H., Ph.D., Professor, Zoology, Connecticut College, New London, Conn.

De Garis, Charles F., M.D., Ph.D., Professor, Anatomy, University of Oklahoma School of Medicine, Oklahoma City, Okla.

Ferry, Ronald, M., M.D., Associate Professor, Physical Chemistry, Harvard University, Boston, Mass.

Gilbert, Max., M.D., Ph.D., Medical Research Division, Schering Corp., Bloomfield, N. J.

Glick, David, Ph.D., Chief Chemist, Beth Israel Hospital, Newark, N. J.

Hadley, Philip Bardwell, Ph.D., Professor, Physiology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Hammett, Frederick S., Ph.D., Scientific Director, The Lankenau Hospital, Provincetown, Mass.

Harrington, Marshall C., Ph.D., Associate Professor, Physics, Drew University, Madison, N. J.

Hartman, Frank Alexander, Ph.D., Professor, Physiology, Hamilton Hall, Ohio State University, Columbus, Ohio.

Harvey, E. Newton, Ph.D., Professor, Biology, Princeton University, Princeton, N. J.

Hill, Edgar S., Ph.D., Instructor, Biochemistry, Washington University, School of Medicine, St. Louis, Mo.

Jones, Howard W., A.M., President, Youngstown College, Youngstown, Ohio.

Kelser, Lieut. Col. Raymond A., D.V.M., Ph.D., Surgeon General's Office, U. S. Army, Washington, D. C.

Merritt, C. A., Ph.D., Professor, Geology, University of Oklahoma, Norman, Okla.

Metts, Dean F., Crowley, La.

Moneymaker, Berlen C., M.S., Geologist, Oliver Springs, Tenn.

Nelimark, John H., Darby Petroleum Corp., Tuls[illegible] Okla.

Northrop, Stuart A., Ph.D., University of New Mexico, Albuquerque, N. M.

Olmstead, Frank R., Ann Arbor, Mich.

Rodriguez-Molina, Rafael, M.D., Med. Sc.D., Sch[illegible] of Tropical Medicine, San Juan, P. R.

Ruff, Lloyd L., B.S., Geologist, Portland, Ore.

Sawtelle, George, B.S., President, Kirby Petroleum Co., Housto[illegible], Texas.

Washburn, Ruth W., Ph.D., Consultant, Child Development, [illegible]ilton Preparatory School, Milton, Mass.

Watson, Maud E., Ph.D., Director, The Children's Center, D[illegible], Mich.

Willard, Bradford, Ph.D., Geologist, Pennsylvania Topographic and Geologic Survey, Harrisburg, Pa.

Wilson, E. Bright, Jr., Ph.D., Assistant Professor, Chemistry, Harvard University, Cambridge, Mass.

Wilson, James W., Ph.D., Associate Professor, Biology, Brown University, Providence, R. I.

Wilson, Louis Palmer, Ph.D., Instructor, Zoology, Wellesley College, Wellesley, Mass.

Winternitz, M. C., M.D., A.M., Pathologist, New Haven, Conn.

Winton, W. M., M.S., Professor Biology and Geology, Texas Christian University, Fort Worth, Texas.

Withrow, Lloyd, Ph.D., Organic Chemistry Department, General Motors Corp., Detroit, Mich.

Wolfe, Harold R., Ph.D., Zoology, University of Wisconsin, Madison, Wis.

Wolff, Dorothy, Ph.D., Assistant Professor, Applied Anatomy in Otalaryngology, Washington University, St. Louis, Mo.

Wright, W. H., D.V.M., Ph.D., National Institute of Health, Washington, D. C.

STUDENT MEMBER

Merrill, J. Edwerta, A.M., Dept. of Zoology, Barnard College, New York, N. Y.

TRANSACTIONS

OF THE

NEW YORK ACADEMY OF SCIENCES

SERIES II

VOLUME 2

NEW YORK

PUBLISHED BY THE ACADEMY

1939–1940

Associate Editor

EUNICE THOMAS MINER, Executive Secretary

CONTENTS OF SERIES II, VOLUME 2

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 NOVEMBER, 1939 No. 1

SECTION OF GEOLOGY AND MINERALOGY

OCTOBER 2, 1939

MEREDITH E. JOHNSON, State Geologist, Trenton, N. J.: *Unsolved Problems of New Jersey's Geology.* (This lecture was illustrated by lantern slides and motion pictures.)

In 1836, Henry D. Rogers published his first report on the geology of New Jersey and this was followed in 1840 by his "final report." At that time this latter report doubtless *was* the last word on the subject; yet the need for more detailed knowledge of the geology of the State soon became evident, and I do not think anyone would seriously contend that the many reports on geology which followed were not needed, or that they did not help in the development and utilization of New Jersey's natural resources. It is unfortunate that in the last twenty years the idea that New Jersey's geologic problems have all been solved has gained so much ground that even our legislators are laboring under that impression. Perhaps the publication of the geologic map of the State in 1912 did as much as anything to foster this idea. It is a good map, published on a scale of four miles to an inch, and for many purposes it has been entirely adequate. But it does not tell us the chemical variations in the limestones whose areal distribution it shows;

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 2, No. 1, November, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

it does not show where shale suitable for the manufacture of brick occurs; or where material suitable for the manufacture of mineral wool may be found. In short, it does not provide the answer for a great many questions to which increasing industrial development has led; nor does it answer the more academic problems which are of immediate interest to all geologists and the answers to which—if we may judge from past experience—will ultimately prove of practical as well as scientific value. In discussing some of the more interesting of New Jersey's unsolved geologic problems, it is my hope to attract attention to the need for additional geologic work, and the further necessity of making that need known if we are to progress in clearing up our geologic mysteries.

Problem 1. What is the age and what are the relationships of the igneous and metamorphic rocks which cross Delaware River at Trenton?

This is an old and hard nut to crack. The Pennsylvania Survey has been laboring with it mightily for many years, but still the answers to the several questions at issue are not sufficiently clear to be acceptable to all.

The rocks chiefly involved here are of three general types; namely, a light-colored granite-gneiss which I am glad to say is apparently accepted by everyone as Pre-Cambrian in age, and which is of the same general type as similar rock in Maryland which has been given the name, "Baltimore gneiss." North of a pronounced fault which runs through Trenton, Langhorne Station, Trevose and points farther to the southwest, the granite-gneiss is overlain stratigraphically by a hard, typically pale-green quartzite and that by light-colored dolomitic limestone. Though not stratigraphically continuous with the similar sedimentary beds near New Hope and Norristown, the gaps are not wide, and there is general agreement again that all these beds are to be correlated with the Chickies quartzite of Cambrian age, and the Shenandoah or Kittatinny limestone of Cambro-Ordovician age. But there agreement ends, for on the south side of the aforesaid fault we find a metamorphosed sediment—the Wissahickon formation—that

by some is considered of Pre-Cambrian age, and by others, Ordovician.

You are all familiar with the arguments involved. The proponents of the Pre-Cambrian hypothesis explain the absence of the thick Wissahickon formation north of the fault by saying that the present contiguous position of unlike series of rocks is due to a great thrust—the Martic overthrust—which has brought together rocks that formerly were many miles distant from one another, and that either (1) the sea in which the Wissahickon sediments were deposited did not extend as far to the northwest as the line of the fault; or (2) that erosion subsequent to their deposition removed them prior to the deposition of the Paleozoic sediments.

The opponents of the Pre-Cambrian theory point to the fact that the line of the fault is relatively straight; whereas, if the angle of the thrust is anything less than 45 degrees, there should be decided offsets where Neshaminy Creek, Pennypack Creek, Wissahickon Creek and Schuylkill River have cut gorges through the resistant quartzite and granite-gneiss. They point also to the similar rock succession southeast of the Martic overthrust at Avondale where quartzite and marble directly overlie the Baltimore gneiss, and the Wissahickon formation admittedly overlies both.

In rebuttal, proponents of the Pre-Cambrian hypothesis can point out that whereas gabbro of identical appearance cuts both the Baltimore gneiss and Wissahickon formations in widespread intrusions, nowhere can it be found cutting the Chickies and Kittatinny formations, or still younger rocks.

Additional arguments for both sides could be cited, but in view of the many other problems which I wish to mention, I will have to proceed.

Problem 2. Girard Wheeler[1] has recently published a paper in the *Journal of Geology* on "Triassic Fault-line Deflections and Associated Warping" which throws new light on another interesting problem: namely, the peculiar crescentic

[1] Wheeler, Girard. Triassic Fault line Deflections and Associated Warping. *Jour. Geol.* 17 (4): 337–370. May–June, 1939.

shape of the several flows and intrusions of trap adjacent to the western border of the Triassic basin in New Jersey. These flows and dikes had previously been studied by Darton, Kummel and Lewis, but according to Wheeler, previous to his study no one had offered a satisfactory explanation of their shape and structure.

In studying the problem, Wheeler found an apparent relationship between irregularities in the fault boundary and the structure of the adjacent Triassic rocks. Where salients of the older rocks project into the younger, the downfaulted Triassic rocks are warped into anticlines. Opposite re-entrants, synclinal folds occur. The throw of the Triassic rocks opposite salients according to this theory is a minimum, whereas at re-entrants, the maximum throw occurs.

In his paper, Wheeler gives many good sketches to illustrate his theory but not all of these seem to confirm it. For example, on page 353 it will be noted that the prongs of the Sand Brook crescent do not face the re-entrant in accordance with theory.

The small basaltic sheet at Flemington is an area of trap which Wheeler did not discuss but which is of interest because of the surprising thickness of trap recorded in the well which is shown in the southern part of the area. This well starts at elevation 263 and not more than 45 feet above the lower contact of the trap with the surrounding shale; yet this same contact was not reached in the well until the drill had reached a depth of 178 feet. Since nowhere along its outcrop does the trap reach such low elevation, the conclusion seems inescapable that the under contact is dipping inward from all sides. This cone-shaped structure is interpreted as the remnant of a small basaltic plug which probably is connected at depth with the diabase dikes which outcrop nearby.

In the anticlinal trap sheet at New Vernon, again there is poor agreement with Wheeler's theory, yet, as he has pointed out, it may well be that beneath the present erosion surface, the fault line has greater convexity.

Wheeler did not discuss the structure of the diabase sill constituting Cushetunk Mountain, but it is so similar in shape and manner of occurrence to the sills and sheets he did describe that it certainly is part and parcel of the same general problem. In this case the trap cuts across the enclosing beds of shale and even close to the great boundary fault the dips seem to bear little relation to it. Is it just coincidence that this great sill was intruded in the form of a crescent with a detached small central plug, or does the boundary fault here too bulge outward at depth in a manner not shown at the surface; and did that bulge antedate the intrusion of the diabase and influence its disposition?

Problem 3. Frequently I am asked to classify specimens of hard, gray, argillaceous rock which at first glance may be either metamorphosed shale, or argillite. The occasional difficulty in giving a satisfactory reply always reminds me of another problem: namely,—What differences in character of sediment or in environmental conditions produced argillite from the muds deposited in the central part of the Triassic basin; whereas the muds deposited to the northeast and southwest were converted into relatively soft shale? Was there actually a difference in the materials deposited? Was there a difference in the chemical content of the ground water which circulated through the different parts of the Triassic sediments; or is some other cause responsible for the great difference in the present hardness of these similar argillaceous beds?

Problem 4. Where is the contact in New Jersey between Cambrian and Ordovician strata?

We know that the Conococheaque limestone of Pennsylvania, which seems to be the correlative of the middle portion of our Kittatinny formation, contains Cambrian fossils—and indeed a few such fossils have been found in beds in the lower and middle parts of the Kittatinny in New Jersey—but fossils in these beds are so scarce that to date we have had little success in attempting to pick out the uppermost bed of Cambrian age.

The same story is true of the overlying dolomitic beds of Beekmantown age. Occasional fossils have been found in the lower part of these beds, but they are few and far between, so the most that we can say at present is that apparently the Beekmantown formation is at least 1700 feet thick in the vicinity of Phillipsburg. In that area, John Hills, while at Lafayette and in subsequent post-graduate work, made stratigraphic studies which included a microscopic examination of limestone residues. To him should go the credit, I believe, for first noting the presence in such residues of very simple forms of foraminifera, and this discovery should serve to help unravel this problem. Dr. Benjamin Howell of Princeton has for several years been working on this same problem and it is hoped that eventually, by the use of all possible aids, we may be able to establish enough criteria to enable us to recognize the correct stratigraphic horizon even of isolated outcrops.

Problem 5. Where should we draw the line between Silurian and Devonian strata in New Jersey?

These strata are well exposed in only a few localities in the northwestern part of the State where they were first studied in detail by Stuart Weller about 1900. In his report on Paleozoic Paleontology he stated clearly that the faunal zones he had found were similar to those in southeastern New York and he followed the New York nomenclature in naming the stratigraphic units. In placing the line between the Silurian and Devonian strata at the contact between the Manlius and Coeymans formations, as had been done in New York, he did not overlook the peculiar Rondout fauna, composed almost exclusively of members of the genus Leperditia, or the abundance of ostracodes in the Manlius and Decker beds. Instead, he did so because of his expressed opinion that "the greatest faunal change is to be found in passing from the Manlius to the Coeymans limestone, and the Coeymans fauna represents the first distinctive immigration of an important, typically marine fauna into the northern portion of the Cumberland basin since its occupation by the Decker Ferry faunas. The importance of

this immigration seems to be sufficient to be recognized as the beginning of the period which we call Devonian time, the Coeymans fauna being the earliest of the Helderbergian faunas in America, all of which have distinctly Devonian characteristics."

I believe that the New York Geological Survey still concurs in Weller's opinion. Recently, however, the Pennsylvania Survey has published "A Paleozoic Section at Delaware Water Gap" by Bradford Willard in which the Decker, Rondout and Manlius are all placed in the Devonian although the Silurian-Devonian contact was admittedly "still a bone of contention in Pennsylvania."[2]

In this connection, Frank C. Whitmore, Jr., who recently finished a very thorough re-study of the fossiliferous beds at the Nearpass quarry in Sussex County, has written me as follows: "In view of this confusion (with respect to the Silurian-Devonian contact), I decided to go as far as I could on what evidence I could find myself. Of course, the age of the Manlius, Rondout and Decker is dependent upon the age of the Keyser, with which it is undoubtedly roughly equivalent; and it is my opinion that the Keyser faunas show a Silurian tendency. I believe that a Silurian age for the Keyser is further indicated by the widespread unconformity in Pennsylvania, Maryland, etc., between the Keyser and the overlying Coeymans. Needless to say, I do not regard the problem as solved, as I know that excellent faunal arguments can be formulated in support of either the Silurian or Devonian age of the Keyser. I drew my own conclusions from what seemed to me to be the strongest faunal evidence; especially that of the ostracoda."

To the writer, the evidence of a stratigraphic break at the top of the Keyser, together with the accumulated faunal evidence, point to the ultimate recognition of the Coeymans as the basal Devonian formation; but the problem certainly cannot yet be called "settled."

2 Since the oral presentation of this paper, Dr. Willard has informed the writer that the Pennsylvania Survey, in its forthcoming bulletin on the Devonian strata of Pennsylvania, has placed these beds in the Silurian.

Problem 6. What is the age of the Manasquan, Vincentown and Hornerstown formations?

William B. Clark was the first to classify the coastal plain sediments on the basis of their contained fossils, and he included everything from the Manasquan down in the Cretaceous. In 1907, after several years of intensive field work, Stuart Weller published his excellent and thorough treatise on the "Cretaceous Paleontology of New Jersey." In this he found occasion to differ somewhat with the stratigraphic divisions made by Clark, but he concurred with him in placing the Manasquan and all older unconsolidated sediments in the Cretaceous, even though the marked unconformity at the base of the Hornerstown marl was then known. It was not until 1928 that this classification was seriously questioned. In that year, however, C. W. Cooke and Lloyd W. Stephenson[3] made a new analysis of the fauna from the Hornerstown, Vincentown, Manasquan and Shark River formations which, together with the aforementioned unconformity at the base of the Hornerstown, seemed to strongly indicate the Eocene age of all four formations.

In 1930, Alexander Wetmore[4] reviewed the stratigraphic occurrence of fossil birds from supposed Cretaceous beds in New Jersey and concluded that all the species known were of Eocene aspect, and, hence, that the Hornerstown marl, in which they had all been found, was of Eocene age. This confirmation of Cooke and Stephenson's conclusions was very pleasing to all those who wished to see this problem definitely settled; but in 1933 the applecart was again upset when Ferdinand Canu and Ray S. Bassler,[5] after a study of the bryozoan fauna of the Vincentown formation, stated their opinion that these bryozoa were more like the Upper Cretaceous than the Eocene forms

[3] Cooke, C. Wythe, and Lloyd W. Stephenson. The Eocene Age of the Supposed Late Upper Cretaceous Greensand Marls of New Jersey. *Jour. Geol.* 36 (2). Feb.–March, 1928.

[4] Wetmore, Alexander. The Age of the Supposed Cretaceous Birds from New Jersey. *The Auk* 47 (2): 182–186. 1930.

[5] Canu, Ferdinand, and Ray S. Bassler. The Bryozoan Fauna of the Vincentown Limesand. U. S. Nat. Mus., Bull. 165. 1933.

of Europe and that they bore little resemblance to the usual Tertiary faunas of America.

Since then, A. K. Miller and M. L. Thompson[6] have lined up on the Tertiary side of the argument as a result of their study of the nautiloid genus *Aturoidea* in America; and Philip H. Jennings[7] has added confirmatory evidence as a result of his study of microfauna from the Monmouth group (Red Bank, Navesink and Mount Laurel-Wenonah formations) and the Hornerstown marl. Also, Stephenson[8] has cited additional faunal evidence in support of his case as the result of a re-study of boring mollusks found in the Vincentown and subjacent formations.

To an impartial geologist, whose only interest is in seeing the problem settled one way or another, the preponderance of evidence seems to favor the supposition that the Hornerstown, Vincentown and Manasquan formations are Eocene in age; but he has recently been reminded that certain dinosaur remains were found in these beds and that the dinosaurs are supposed to have become extinct in late Cretaceous time. So the problem is evidently still far from settled.

This paper could be greatly extended by discussing all of the following problems, but as my intention was only to arouse an interest in their settlement, I will be content merely to list them:

Problem 7. What is the age of the Manhattan schist formerly exposed at low tide in Jersey City, and of the serpentine which apparently intrudes it?

Problem 8. To what extent has thrust faulting displaced the strata of the Highlands region? For example, we know that at West Portal the Pre-Cambrian gneiss has been thrust northwestward for at least one mile. Is this a maximum, or

[6] Miller, A. K., and M. L. Thompson. The Nautiloid Genus *Aturoidea* in America. *Jour. Pal.* 9 (7): 563–571. 1935.

[7] Jennings, Philip H. A Microfauna from the Monmouth and Basal Rancocas Groups of New Jersey. *Bull. Amer. Pal.* 23 (78): 3–76. 1936.

[8] Stephenson, Lloyd W. The Stratigraphic Significance of *Kummelia*, a New Eocene Bivalve Genus from New Jersey. *Jour. Wash. Acad. Sci.* 27 (2): 58–64. 1937.

is the throw still greater at other points where evidence bearing on the problem is now lacking?

Problem 9. What forces produced the complicated structural pattern north of Flemington, where the Triassic and Paleozoic sediments have been twisted around so that they strike at right angles to the normal NE-SW structural trend?

Problem 10. How far to the southeast do the Paleozoic strata extend beneath their present cover of Triassic sediments?

Problem 11. What is the genesis of the zinc deposits at Franklin and Ogdensburg?

Problem 12. What was the source of the copper minerals at the old Pahaquarry Mine near Dimmicks Ferry, Delaware River, which is many miles distant from the nearest known intrusion of igneous rock? In what age did mineralization occur?

Problem 13. What is the origin of the depressions in the relatively flat surface of the Pensauken formation between Trenton and South Amboy, and occasionally seen in level ground, underlain by other formations?

Problem 14. What is the age and origin of the Cape May formation, the Pensauken, the Bridgeton, and the Beacon Hill?

Problem 15. How many glacial ice-sheets over-ran northern New Jersey? Did the ice-front slowly recede at the end of each glacial epoch with occasional halts and re-advances, as believed by Salisbury and Antevs; or was the maximum advance followed in each case by stagnation of the ice-sheet with gradual disappearance due to melting in place, as believed by Flint?

Problem 16. What was the maximum westward extension of the sediments of the Coastal Plain?

Problem 17. Do the headwaters of any of our major streams antedate the Cretaceous sediments, and if so, which ones?

Problem 18. How many peneplains have been developed in New Jersey and what is the evidence?

These are some of the more important unsolved geologic problems that have occurred to me, but I make no pretensions that the list is complete. Probably any one familiar with the geology of New Jersey could name half a dozen more without much thought. For the sake of brevity, however, I shall bring this paper to an end with a brief statement of what is being done to solve one final and related group of problems: namely,—

Problem 19. What is the structure of the sediments constituting the Coastal Plain? What is the depth to the rock floor beneath the sediments? Of what kinds of rock is the rock floor composed?

The best way to get data about materials below the surface of the ground is to drill test holes. By preserving samples of the materials encountered and keeping a careful record of the depth at which each change occurs, an accurate picture of the underground strata can be obtained. For that reason the New Jersey Geological Survey has from its inception attempted to obtain and preserve samples from deep wells in areas where information pertaining to the sub-surface geology is lacking. But deep wells are expensive, and unless there is some practical purpose to be served, such as the obtaining of a groundwater supply, no drilling is done and the geologist must seek his information in other ways. That is why geophysics has come to play an increasingly important role as an aid to the geologist; for information can now be obtained by geophysical methods which would be exorbitantly expensive if it had to be obtained by drilling.

Because the evidence accumulated from wells in recent years had thrown some doubt on the old conception that the sediments of the Coastal Plain had undeformed, monoclinal dips to the southeast, the opportunity to aid Dr. Maurice Ewing and Dr. George P. Woollard in the fall of 1937 in making a survey by seismic methods of a line across the Coastal Plain was eagerly accepted. It was thought desirable in running this first seismic profile to have an indisputable check upon the results. Consequently, it was run through Jacksons Mills where

a deep well had been drilled far into the basement rock. Also, it was run at right angles to the strike of the sediments in order to determine their maximum dip. As fully described in papers which have appeared in the *Transactions of the American Geophysical Union*[9] and the *Bulletin of the Geological Society of America*,[10] the results of the survey were most gratifying. Not only was the dip of certain recognizable horizons determined accurately, but also the depth to the rock floor was determined for the entire line of the profile; a hitherto unsuspected fault block was mapped near Plainsboro; and the nature of the underlying crystalline rock was determined on the basis of rock densities and structural trends.

A second seismic profile across the Coastal Plain was run in the summer of 1938 from Bridgeport to Avalon in Cape May County; and this line also was run at right angles with the regional structure. Results again were highly satisfactory, although inability to reach basement with the shots at Seaville and Avalon was somewhat disappointing. This work showed an average dip of the rock floor of about 85 feet per mile as compared with 65 feet per mile along the more northerly profile, as well as the same steepening of the dip near the present shoreline. The depth to basement at Woodbine, the most easterly of the stations to reach bed rock, was determined to be 4,592 feet. For other results, I am going to refer those who read this to Part IV of "Geophysical Investigations in the Emerged and Submerged Atlantic Coastal Plain," which is due to appear soon in the Bulletin of the Geological Society of America.

Another geophysical aid was invoked through the U. S. Coast and Geodetic Survey, who very generously extended their net of gravity stations in northern New Jersey in 1937, and in 1938 occupied stations along the lines of both the afore-

[9] Woollard, George P., Maurice Ewing and Meredith E. Johnson. Geophysical Investigations of the Geologic Structure of the Coastal Plain. *Trans. Amer. Geophys. Union.* Nineteenth Annual Meeting. 1938.

[10] Ewing, Maurice, George P. Woollard and A. C. Vine. Geophysical Investigations in the Emerged and Submerged Atlantic Coastal Plain, Part III. Barnegat Bay, New Jersey, Section. *Geol. Soc. Amer. Bull.* 50: 257–296. 1939.

mentioned seismic profiles as well as certain detached stations which had been requested. The results of this gravity work have been plotted through points having the same gravity anomaly, and I think that I am safe from contradiction in saying that some of the anomalies shown are very suggestive. For example, the lines denoting equal anomalies which run from Bordentown to the northeastern corner of the State come very close to following the eastern boundary of the Triassic rocks. Likewise, the high negative anomalies observed in eastern Pennsylvania and Vermont appear to be directly related to the steeply folded Paleozoic rocks of the Appalachian geosyncline. The change to positive anomalies in the central part of New Jersey would seem to indicate the presence of high-density rocks such as the gabbro which has so permeated the Wissahickon formation. Other inferences are possible, but the relation between structure and gravity is not always clear and I will, therefore, let my readers draw their own conclusions as to the significance of the high negative anomalies in Cape May County and the possible relationship between the closely spaced isostatic anomalies passing through Philadelphia and Trenton,—representing a rapid transition from positive to negative anomalies—and the earthquake which severely shook those cities a year or so ago.

Finally, during this past summer, one other geophysical aid was invoked. With an Askania magnetometer and such aid as the writer could find the time to give, George Woollard has mapped the position of the buried Palisades diabase between Sayreville and Deans, and has extended the network of his magnetometer readings both to a considerable area in the neighborhood of Plainsboro and Princeton Junction, and also along the northerly profile from Plainsboro to Mantoloking and beyond. These readings of the vertical intensity of the earth's magnetic field should help materially in the interpretation of the gravity and seismic data already obtained, and I am going to suggest that if this Section of the Academy can get Woollard to explain the significance of his field data at some future meeting, it will be rewarded by a most interesting paper.

SECTION OF BIOLOGY

OCTOBER 9, 1939

JEROME ALEXANDER, Consulting Chemist, New York, N. Y.: *Physico-chemical Determinism in Biology.* (This lecture was illustrated by the use of opaque projection apparatus and slides.)

Berzelius' view that catalysts owe their potency to what we now term their outwardly directed residual electronic fields, is a basis for understanding the extreme specificity of catalysts in general and of enzymic biocatalysts in particular. The potency of molecular electronic fields is great, but highly localized, varying approximately as the ninth power of the distance (Langmuir). Before reactants can be sufficiently affected by the electronic fields of a catalyst, they must enter its electronic jurisdiction, near enough to be distorted by these fields of force, somewhat as the opposing hooks of a "zipper" are twisted into position to unite by the "key."

Commercially, catalysts are used to synthesize a wide variety of organic and inorganic compounds on an enormous scale. The kind of catalyst used determines the nature and percentage of substances synthesized. Introduction of new atoms or molecules into the catalyst surface may stimulate, inhibit, or alter the nature and rate of the catalyzed reactions. Generally, any substance which changes the behavior of a catalyst may be called a "modifier," and exceedingly minute amounts of materials serve this purpose.

Up to a certain point, increase in temperature will facilitate reaction by bringing to the catalyst more reacting particles per unit of time but beyond this point the number of particles exceeding the critical speed limit will increase so greatly that catalyst efficiency will fall off; hence the optimum temperature of action so commonly observed for biocatalysts.

The genes and enzymes which largely direct the course of chemical change in living beings are catalyst particles of col-

loidal dimensions. Some molecules may be of such size that they are born colloids; some achieve colloidality by molecular growth or by aggregation; and some have colloidality thrust upon them by adsorption or chemical fixation on a colloid particle or surface. If the fixed particle can form a specifically active catalytic area after fixation, it is termed a *prosthetic* or activating group. The word "prosthetic" is derived from a Greek root meaning to add to, or to insert. Thus prosthetic dentistry deals with the insertion of teeth to replace missing ones. A prosthetic group puts "teeth" into the catalyst.

Decrease in the size of a particle results in a great increase in its kinetic activity. Toward the lower range of the colloidal zone where the specific surface (free surface per unit weight) is still considerable, particulate kinetic activity becomes ultramicroscopically visible; whereas to use an "Irish bull," if we could see a hydrogen molecule we could not see it, because it would move too fast—with about the speed of a rifle bullet. That degree of smallness of particle where kinetic activity is insufficient to nullify or to obscure the consequences of specific surface, has been termed the *zone of maximum colloidality.*

Life involves two essential activities: self duplication or reproduction and the catalytic direction of chemical change. These two criteria would be met by an autocatalytic catalyst, that is, a catalyst which catalyzes its own formation. In addition, most, if not all living units that we know, are capable of undergoing changes (*e.g.,* differentiation, mutation) which alter the nature of their catalyzed or metabolic products, and which may be passed on to their descendants. Genes, bacteriophages and viruses are examples of the smallest known living units. They are sub-microscopic colloidal particles of widely varying dimensions, evidently extremely large molecules or comparatively small molecular groups. Given a suitable milieu, such units reproduce and direct chemical change. According to this view, an enzyme capable of self-duplication would have to be considered alive, and the simplest conceivable living unit would be a *moleculobiont,* (Alexander & Bridges, 1928) a molecular

catalyst, which, given a suitable milieu, is capable of self-duplication. When we have not succeeded in finding a laboratory milieu in which to propagate a biont which grows parasitically upon or within other bionts, it is common to refer to the invader as an *obligate parasite.* The term *obligate* is merely a confession of temporary ignorance, and not the expression of an experimentally warranted conclusion; for the production of suitable synthetic media should be regarded as unsolved rather than insoluble problems.

To understand the ultimate basis of life within a cell, we must concentrate our attention upon the simplest self-duplicating units—chromosomes and their constituent genes, mitochondria and possible sub-cellular symbionts. The cytoplasm, apart from these, is a pool of localized milieu, receiving and contributing special molecules, an arena for the activities of the enzymes and catalytic surfaces. The ultimate living units in a cell are the self-duplicating ones. The view that protoplasm is the "living jelly" is a relict of superannuated textbooks, and should be definitely abandoned.

Both enzymes and viruses have been crystallized and appear to be proteins. In their natural state they are dispersed colloids and are probably prevented from exercising their natural tendency to establish a visible crystal lattice by the presence of protective or deflocculating colloids and by the other conditions (*e.g.,* ionic concentrations) of their natural milieu. Special procedures are required to purify and crystallize them. The crystallization of substances heretofore believed to be non-crystallizable has astonished many who fail to understand that orderly arrangement in a space lattice is a potential minimum which molecules and molecular groupings tend to assume, although circumstances prevent, and some intermediate stage persists. We have many crystalline colloids—Scherrer showed that the individual particles of colloidal gold are tiny crystals.

Proteins are subject to aggregative changes apart from actual crystallization. Thus Eriksson-Quensel and Svedberg found that the molecules in a solution of the hemocyanin from

the snail *Helix pomatia,* show a molecular weight of 6,740,000 when slightly acid (pH 6.8); but when made slightly alkaline (pH 8.0) the protein splits into three components whose molecular weights are 6,740,000, 3,370,000 and 842,000 respectively. On restoring the pH to 6.8, all the fragments of dissociation completely unite to form the original component. All vertebrates except the Cyclostomata have a protein blood corpuscle pigment of the same molecular weight, 68,000 (hemoglobin) with four iron atoms per molecule. With Cyclostomata the corpuscle pigment has a molecular weight only one-fourth that of hemoglobin. "The molecular weights of the hemocyanin molecules found in the blood of certain species are always simple multiples of the lowest well-defined component. Thus, for the Malacostraca the relationship is 1:2 and for the Gastropoda 2:8:16:24. Moreover, the weights of all the well-defined hemocyanin molecules seem to be simple multiples of the lowest among them. In most cases the hemocyanin components of certain species are interconnected by reversible, pH-influenced dissociation-association reactions. The shift of pH necessary to bring about reaction is not more than a few tenths of a unit. Consequently, the forces holding dissociable parts of the molecules together must be very feeble."

"Not only the molecular weights of the hemocyanins but also the mass of most protein molecules—even those belonging to chemically different substances—show a similar relationship. Probably the protein molecule is built up by successive aggregation of definite units, but only a few aggregates are stable. The higher the molecular weight the fewer are the possibilities of stable aggregation. The steps between the existing molecules, therefore, become larger and larger as the weight increases." (Svedberg, *Ind. Eng. Chem., Anal. Ed.,* 1938, **10**, 113–129.)

Since few, if any, biological substances above the molecular or macromolecular orders occur in a state of purity, we must consider some aspects of the behavior of mixtures as contrasted with the behavior of pure substances. Mere traces, too often regarded as "negligible impurities" or as the "undetermined"

amount necessary to bring an analysis to 100 per cent, may exert surprising influence, apart from serving as prosthetic groups in enzymes. Such secondary constituents, adsorbed at specific particulate surfaces, often act as protective or deflocculating colloids, opposing the formation of larger aggregates. Frequently, however, they may cause the coated particles to *cohere,* and may then be termed *cohesive colloids.*

With very small surfaces chemical specificity becomes increasingly dominant. Thus adsorption of antibody by antigen is highly specific, though there may be some overlapping or cross-precipitation. Where bacteria become coated with an adsorbed layer of their specific antibody, and then approach closely, the antibody serves as a cohesive colloid and binds the bacteria into a clump of such size that Brownian motion ceases and the clumps gather together and flock out. Topley, Wilson and Duncan showed that on agglutinating a heterogeneous mixture of bacteria by a heterogeneous mixture of specific antisera, each cluster of bacteria is homogeneous. Lindegren and Bridges invoke this principle to explain synapsis, which "would proceed zipper-like, throughout the entire length of the chromosome."

Without the intervention of some mechanism capable of directing orderly differentiation, the rapidly segmenting zygote would become a mass of cells of one kind. The formation of a globular mass of cells establishes a *diffusion gradient* between cells on the interior. The external cells are more advantageously situated for chemical interchange with the milieu and tend to duplicate more rapidly than the cells blanketed within the mass. This means that the cell mass must develop more external surface, which, unless the cells disaggregate, can happen only in one of the two ways: (1) the surface cells may break loose from the main mass and form a *diverticulum,* as is common with plants, *e.g.,* in roots and hyphae; (2) if the cells cohere, the struggle for additional free surface leads to an *invagination,* and we have the gastrula form characteristic of animals. The development of a gastrula automatically en-

forces the development of three ionic domains: (1) an ectoderm, in contact with the external "ocean" of milieu; (2) an endoderm, in contact with the gastrulitic "gulf or bay"; (3) a mesoderm, intermediate between the other two domains. With the establishment of these ionic differences we can understand how specific molecules can be selectively aggregated or adsorbed by cellular catalysts within the cells of the several domains. The catalysts (genes and/or enzymes) *thus undergo modification* and the chemism of the cells undergoes a corresponding diversion, which leads up to and becomes evident as differentiation. The modifying particles apparently accumulate in the so-called gray crescent at the mouth of the gastrula, from which, as Jennings puts it, an organizing influence sets out. The relation to the *organizator* of Spemann and the evoker of Needham is at once evident. The specific molecules needed for the modification of the cellular catalysts at this stage, though large in number, are so small in mass that they would readily have been carried in the cytoplasm of the original zygote. Though there are billions of diverse but highly specific molecules free within the zygote cytoplasm or adherent to the chromonema, it is not necessary to assume that these must be sufficient to modify all the catalysts in larger adult organisms. The specific modifying particles of the zygote may serve as moulds or templets for the formation of duplicates of themselves. A self-duplication of this kind evidently occurs with bacteriophage, viruses, and mosaics, though some of these may have a supermolecular structure. To illustrate this mechanism, a piece of metal foil is pressed to the surface of a coin. The foil on the face *next* to the coin acquires a surface contour precisely opposite to that of the coin; but the outer surface of the foil shows a surface contour *precisely the same* as the coin. Thus a catalytic mould could duplicate its own specific surface as well as produce a specifically reverse surface, depending upon which surface remains facing the milieu following adsorptive or other fixation of the liberated molecular or macromolecular plaque. The work of Langmuir, Adam, Wrinch and others shows the thinness and top-and-bottom specificity of surface films.

If the normal course of life is in part directed by normal catalyst modifiers, the introduction of abnormal modifiers may so divert the course of catalysis as to give rise to functions or structures which could be termed pathological. For example in methylcholanthrene we have a definite chemical substance which incites cells to duplicate themselves and to invade healthy tissue, a condition which clinicians identify as cancer. Since cancer cells duplicate themselves specifically, the catalysts and/or modifiers responsible for abnormal cell behavior must also be duplicated. Whatever the remote cause, the proximate cause of cancer appears to be a heritable change in biocatalysts either by way of mutation or of modification.

SECTION OF PSYCHOLOGY

OCTOBER 16, 1939

PROFESSOR HORACE B. ENGLISH, Ohio State University, Columbus, Ohio: *Experiments in Substance Memory.* (This lecture was illustrated by lantern slides.)

The learning of everyday life consists in large part of acquiring the "substance" or meaning of something studied. This type of learning is little investigated and even little illuminated by traditional experimental procedures. This is partly because traditional theory minimizes meaningfulness, partly because it is difficult to experiment with meaning. For as soon as one contrasts something having less meaning—rote memory material, say—with something having rich meaning, one introduces other differences of unknown effect on the learning process.

We have sought to avoid this difficulty by contrasting as principal experimental variables, not materials with different degrees of meaningfulness, but two kinds of learning outcomes which reveal—and require—different degrees of acquisition of meaning.

Specifically, we use a battery of recognition questions. Half of these can be answered in virtue of a purely verbatim, rather rote-like retention. The other half can only be answered if one has grasped the meaning or substance of the prose passage used. Both sets of questions are based on the same passage. One uses the same subjects under the same learning conditions for both sets of questions.

Over two million individual responses have been recorded under a variety of experimental conditions. Special attention has been focused on the phenomenon of reminiscence over periods ranging from ten minutes to ninety days. This paper deals with a methodological problem involved in one typical experiment.

Reminiscence seems to play a more significant rôle with connected prose than with nonsense syllables. Average scores based on the total test usually show a rise after twenty-four hours, followed by a slow decline. When the test is divided into its two components, the test requiring the greater amount of meaning-understanding shows no forgetting; instead there is slight but consistent gain. Apparently rote memory is subject to much forgetting, grasp of significance to none at all within the ninety day period. All the forgetting in average scores is localized in the half of the test which rests primarily on verbatim retention.

This, however, is a matter of averages. But forgetting and reminiscence are matters of the patterns of individual responses to specific memory tasks—in this case of the permutations of right and wrong responses by a particular subject to a particular test item at different times. For this the statistics of central tendency are misleading; we need what we may call statistics of categories. Tabulation of the response-patterns in a four way table, Forgetting-Reminiscence and "Much Meaning"-"Less Meaning" still showed forgetting correlated with the more rote-like memory task.

Since, however, reminiscence and forgetting are relative to the earlier response, we should count, not the absolute, but the relative amount of reminiscence in the two halves of the test. And by the criterion of per cent of possible chances, almost exactly as many items are forgotten in each sub-test—a conclusion almost directly opposite to that previously reached.

Further analysis to resolve the contradiction took the form of a simplified analysis of the variance to eliminate the intrusive effect of differential initial difficulty. By this means, it is shown that the correlation between reminiscence and meaningfulness holds after all. This conclusion was confirmed in a later study in which initial difficulty was held constant not merely statistically but experimentally.

The need for a methodological critique is clear if we review the series of conclusions and their bases:

1. Logical memory shows first a rise and then loss with lapse of time. (Conclusion based on total test averages.)
2. Retention of just the substance or idea of a passage shows no loss with time but rather reminiscence. All the forgetting found in the total test is localized in the half-test which stresses verbatim retention. (Based on sub-test averages.)
3. There is the same *relative* amount of forgetting in the two sub-tests. (Based on the per cent of possible manifestations.)
4. There is an equal amount of forgetting in the two sub-tests, but more reminiscence in one than the other. (Based on an analysis of individual patterns of response, equating for initial difficulty.)

But the more careful the analysis, the more certain it seems to be that the two sub-tests are really measuring distinct processes. The consistency of the association or correlation of discrepant retention values with relative meaningfulness is most parsimoniously explained in terms of at least two distinct, though probably inseparably linked, organic processes; the one process mediating a rote-like memory process and conditioning, the other mediating the development of meaningful understanding.

Whether, however, this hypothesis is ultimately confirmed or not, it is contended that on the merely descriptive level there are disclosed too many differences between richly meaningful memory and the meaning-impoverished memory of traditional experiment for the latter to be considered the pattern of the former. On both theoretical and practical grounds the time has come when the kind of learning most used in everyday life (the learning of the "substance" of what is presented) must be systematically investigated.

SECTION OF ANTHROPOLOGY

October 23, 1939

Doctor Margaret Mead, American Museum of Natural History: *Researches in Bali, 1936–1939*. (This lecture was illustrated by motion pictures.)

I. *On the Concept of Plot in Culture*

Anthropology is such a young science and its materials are so rich and varied that the study of almost every culture suggests to the investigator new approaches to the material, and approaches which are often not immediately applicable to other cultures. Although eventually every approach, every method of analysis which has proved fruitful in the study of a particular culture must be systematized so that it has cross cultural significance, and the absence of a special type of relationship between cultural phenomena must be integrated with the presence of this relationship elsewhere, the fact that this systematization is not yet possible need not discourage us from using as completely as possible the special leads which given cultures provide. Bali is an extremely rich culture, rich in symbolic forms which may be studied in relation to the type of personality which they express on the one hand, and help to create on the other.

Certain forms of trance in Bali illustrate particularly well one of these special approaches to the analysis of cultural forms, the idea of "plot," originally sketched out in almost allegorical terms by Freud,[1] and later reduced to terms more consistent with cultural diversity by Roheim.[2] This approach suggests a close and patterned relationship between a type of experience in early childhood, which is standardized for a given culture, and the ritual or other symbolic forms found in the same cul-

[1] Freud, S. *Totem and Taboo.* New York: Moffat, Yard and Co., 1918.

[2] Roheim, G. *The Riddle of the Sphinx.* London: The Hogarth Press, 1934. See also Mead, M. Review of the above in *Character and Personality*, September, 1935.

ture. This conception proves exceptionally fruitful in some cases; we do not yet know whether it is applicable to all cultures and caution must be observed even in its application to the analysis of a given culture.

Bali shares with many other regions of the world an emphasis upon trance states, as a way in which the will of extra-natural beings, the dead and the Gods, communicate through possessed persons with human beings. In many of its forms Balinese trance does not differ in any conspicuous way from trance in other parts of Oceania and even in other parts of the world. It is possible to distinguish various types of Balinese trance, from the simple spontaneous trance in which a private, unsanctified and uninstitutionalized person falls down in a faint and makes associated comments which may or may not be accepted as having supernatural import, through institutionalized seers and oracles, who play an important rôle in providing the slight impetus towards change, towards the adoption of new forms of rituals, building new temples, consecrating new religious officials, etc., which is permitted within the rigid and static structure of Balinese society. There is another important variety of trance, in which the performers are called *sangijangs,* and go into states preceded and followed by short deep trances, during which, in a condition which seems to parallel somnambulistic states induced by hypnosis, they perform a variety of stereotyped dances and dramatic representations. Although the details of the *sangijang* forms reflect important aspects of Balinese culture, they apparently do not necessarily depend upon any particular traumatic childhood experience, and would presumably be possible to any child who had been exposed to the general impact of Balinese culture. I mean exposure in the sense that Chinese and Europeans living in Malay regions sometimes manifest the peculiar form of madness known as *amok* which is characteristic of that region of the world.[3] The proportion of individuals able to attain these special states,

[3] van Wulfften Palthe, G. Psychiatry and Neurology in the Tropics. *The Malayan Medical Journal,* September, 1933.

although heavily dependent upon local styles (*e.g.:* In the ward of one large city almost every girl had been in trance at sometime during her childhood or young girlhood. In another village trials over two months failed to find a single suitable girl who would go into trance when presented with the customary stimuli), does not seem to differ radically from such distribution in other regions where trance has been institutionalized.

There is, however, one form of Balinese trance, which appears to bear a close relationship to a definite type of childhood experience in the child-mother relationship which is characteristic of Balinese culture—this is the so-called "kris dance" which is typically brought on by a whole or partial reenactment of the *Tjalonarang* story. Careful records, in still photographs, in cinema, and verbal records, have been collected to show this basic mother-child situation in which the mother over-stimulates the young child, by setting up jealousy and coquetry situations, borrowing younger babies and suckling them to arouse jealousy, placing younger babies on their own children's heads, stimulating their children sexually, and at the same time offering no emotional rapport in return. The mother teases and flirts with the child until she produces either a state of hysterical delight or of violent weeping, and then, refusing to become involved herself, she turns casually to something else. By the time the Balinese child is three to four years old, it learns not to respond to this one-sided situation; it withdraws more and more into itself; and the basis is formed for the insulated type of personality which is typically Balinese and which fails to enter into close emotional relationship with anyone, relying instead upon ritual and art as a means of emotional expression.

In this particular form of trance, which re-enacts this childhood experience, the preface to the trance is a theatrical performance based on the story of the witch who sends pestilence upon the land; is unsuccessfully attacked by an emissary of the king of the country, who then is transformed into the dragon (*barong*)—a double mask representing a guardian supernatural

potent against disease—who, accompanied by a series of human subjects armed with krises, attacks the witch in her supernatural masked form. The witch does not fight against her attackers. She merely looks at them and they fall to the ground to rise and attack again when she takes her eye off them. They attack her in hand to hand combat, and, indifferent, relaxed, casual—even as the mother who has teased her child into a temper tantrum—she reels back and forth, unhurt by their blows, and they fall down into deep trance, to be revived into a somnambulistic state by the dragon. Then, in a frenzy, they turn their krises, which were powerless against the witch, against their own breasts, pressing the kris points against areas of skin felt to be itching intolerably. Finally they fall to the ground and go again into either a limp or rigid deep trance state from which they must be ceremonially roused with holy water and incense.

The "plot" correspondence here between the childhood experience and the special form of trance is very close and is supported by a variety of materials. But in assessing the theoretical importance of the concept of such plot correspondences it is necessary to bear in mind the probability that (1) standard traumatic experiences may occur in a culture without any such accurate ritual or artistic expression; and (2) ritual forms, originally developed or secondarily altered to correspond with the special culturally standardized childhood experience, may be borrowed and used by peoples whose cultures do not provide for any such childhood experiences. In order to establish, not the origin of the ritual in the childhood experience, but the mere fact of contemporary correspondence between childhood experience and ritual form, it is necessary to have full material on both aspects—the typical experiences of childhood and the ritual in question. There is at present no evidence to show that one can be inferred from the other. Our material suggests strongly that such correspondences as do exist can be most effectively studied by methods which record posture and gesture—*i.e.*, photographic and ciné records and also native art products.

II. *Methods of Research in Bali and New Guinea*

During the period 1936–1939 a group of investigators has been working in close cooperation in Bali:—Gregory Bateson, of Cambridge University; Margaret Mead, of the American Museum of Natural History, from March 1936 to March 1938, and during a check-up period of six weeks in 1939; Miss Jane Belo, during a number of periods of residence in Bali between 1931 and 1939, with intensive work from February 1937 to March 1938, and February 1939 to June 1939; and Mrs. Katharane Mershon, resident in Bali since 1931 and working intensively during 1938–1939.[4]

The work was financed by the following institutions: American Museum of Natural History (South Pacific Exploration Fund and the F. G. Voss Fund for Anthropological and Archaeological Research); National Committee for Mental Hygiene, through a grant from the Committee for Research in Dementia Praecox, founded by the Thirty-Third Degree Scottish Rite, Northern Masonic Jurisdiction, U.S.A.; St. John's College, Cambridge University; The Social Science Research Council (Committee on Grants in Aid); the William Wyse Foundation of Trinity College, and by considerable personal contributions from the participants in the research.

Because Balinese culture is too complex to be studied as a whole as a primitive culture might be studied, several methods of sampling were employed, by locality, by caste, and by concentrating on special rituals, special religious manifestations and special types of artistic productions.

Five localities were studied intensively, one or more of the investigators residing there: Bajoeng Gede, (simple mountain village), Bangli, (capital city of district), Batoean, (a village of high caste artists), Sajan, (a simple plains village) and Sanoer, (a district by the sea). We were assisted by trained Balinese secretaries, who were taught to record all events witnessed in Balinese and were given special training in recording verbatim conversations, in interviewing, in recording texts from dictation, etc. For the current events which were witnessed and recorded, there are at least two accounts, synchronized

[4] This central group of investigators also had the benefit of the cooperation of Dr. R. Goris, specialist in Balinese language and archaeology, Mr. C. J. Grader, specialist in the social organization of Balinese mountain communities, Mr. Colin McPhee, specialist in Balinese music and Mr. Walter Spies, specialist in Balinese arts.

on a time scale, one by a European observer and one by a native observer. For the great proportion of events which took place in the day time, there is a synchronized photographic record, combined of Leica stills and cinematograph records. For large feasts and especially important events we were sometimes able to concentrate the whole group of observers. Uniform methods of recording were used by the whole group. All details of behavior, whether verbally or photographically recorded, are placed in a known setting, against a time scale. In addition to these, cooperative records of current events, feasts, trances, theatrical performances, rites de passage, etc., special bodies of material were collected. The most significant of these are a collection of some 1200 paintings, in a recently developed style, by painters in two villages, made over a period of 2 years, comprising representations of almost every aspect of the culture, and combined with intensive study of many of the artists; a collection of some 800 contemporary carvings from a cluster of mountain villages, collected with the special intention of throwing light about attitudes and conceptions of the human body; collections of children's drawings from the localities studied intensively; and a collection of highly stylized shadow play puppets to serve as control material in comparison with the more modern art forms. In connection with these collections, there are verbal, photographic and ciné records of painters, carvers and puppet makers at work. Texts were collected of the stories upon which the paintings are based. A group of 25 young children in Bajoeng Gede was followed intensively for a period of 22 months and a check-up with full photographic records was made at the end of a further interval of 12 months more. Trancers in a number of villages were studied intensively by Miss Belo, and their records obtained on a sorting test based on Holmgren Wools, the Weigle sorting test and a Balinese object sorting patterned on the Sorting Test B as used by Dr. Bolles.[5] These tests and also the Abel Free Design Test[6] were given to a selected group of native painters. Special comparative studies were made of death rituals in a number of localities and similar cross-locality and cross-caste material was collected on other *rites de passage*. The linguistic materials include over 500 accounts in Balinese by native secretaries of events of which check records are available, and also autobiograph-

[5] Bolles, M. The Basis of Pertinence. *Archives of Psychology*, No. 212.

[6] Abel, T. M. Free Designs of Limited Scope as a Personality Index. *Character and Personality*. September, 1938.

ical texts, records of conversations, records of extemporized repartee in the theatre, trance utterances, etc.

In order to obtain comparable material from another culture, the two years in Bali were followed by an eight and a half month expedition by Mr. Bateson and Dr. Mead to the Iatmul Tribe on the Sepik River in New Guinea, previously studied by Mr. Bateson,[7] where the same methods were used, without the advantages of literate native recording, but supplemented by a great deal more verbal material reflecting the greater verbalism of the Iatmul. The same set of child training situations—suckling, sleeping, bathing, familial constellations in which sibling relationships were involved—material on posture and gesture during carving, painting, in everyday activities, and records of trance behavior were obtained for comparison with the Balinese material.

For two reasons, because of the complexity of the material and because of the war, analysis and publication of many of these results is likely to be delayed for some time. But inquiries will be welcomed from any student who wishes to acquaint himself more fully with the practical or theoretical details of any of the methodologies used, some of which were first used in the study of primitive people on a large scale in the course of these researches. All the materials are deposited in the American Museum of Natural History and are available for inspection. The ciné film used was 16 mm, exposed at 16 except where there was much action, in which case 24 exposures were used. There are 36,000 feet of ciné film and about 43,000 Leica stills.[8] Weaving Leica and ciné into a continuous record is very greatly facilitated by using lenses of the same focal length on Leica and ciné. By alternating the two methods, expense is enormously reduced and the Leicas are available for enlarging. These methods in which a verbal record of behavior over a given stretch of time is supplemented by a full photographic record, are essentially suitable for cooperative work, and cannot be used by single observers without highly trained and literate native assistants, or a very considerable modification in methodology.

PUBLICATIONS TO DATE

1. Abel, T. M. Free Designs of Limited Scope as a Personality Index. *Character and Personality*, September, 1938.

[7] Bateson, G. Social Structure of the Iatmul. *Oceania*, 1932; nos. 3 and 4. Naven. Cambridge University Press, 1936.

[8] This latter number was made possible by buying bulk film and loading into cassetts which reduces the cost to less than a third.

2. Bateson, G. An Old Temple and a New Myth. *Djawa,* publication of the Java Institute, September, 1937.
3. Belo, J. Balinese Children's Drawings. *Djawa,* publication of the Java Institute, Djotjakarta, Java, September, 1937.
4. Mead, M. Public Opinion Mechanisms among Primitive Peoples. *Public Opinion Quarterly,* Princeton, N. J., July, 1937.

Strolling Players in the Mountains of Bali. *Natural History,* New York, January, 1939.

ASTRONOMICAL CONFERENCE

October 20 and 21, 1939

"The Internal Constitution of the Stars"[1]

Doctor Harlow Shapley, Director, Harvard Observatory, was in charge of this meeting as Conference Chairman.

The program comprised the following papers and speakers:—

"The Distribution of Density in Eclipsing Binaries"

Professor Henry Norris Russell, Princeton University
Doctor Theodore E. Sterne, Harvard University
Doctor Zdenek Kopal, Czechoslovakia. Title: "Density Condensations as Inferred from Ellipticities."

"Sources of Stellar Energy"

Doctor Hans A. Bethe, Cornell University
Doctor S. Chandrasekhar, Chicago University
Doctor G. Gamow, George Washington University. Title: "The Energy Sources of Red Giants and the Possible Causes of Cepheid Pulsation."

"Opacity Problems"

Doctor Donald H. Menzel, Harvard University
Professor Philip Morse, Massachusetts Institute of Technology. Title: "The Effects of Chemical Composition on Opacity."
Doctor Jaakko Tuominen, Finland. Title: "Surface Opacity and Central Temperatures of Stars."

The subject of this Conference centered about the fundamental thesis proposed some years ago by President A. Cressy Morrison, to ascertain, if possible, the source of stellar energy. During the past few years, the interest in this thesis has grown to significant proportions, and the best minds in the astronomical field have been engaged upon it. It is a matter of gratification to the Academy that the investigators present included among their number the names most prominent in this field. Under the chairmanship of Doctor Shapley, the papers presented were thoroughly discussed with live interest.

[1] It is expected that a number of these papers will be published shortly in the *Annals* of the Academy (Editor).

It was clearly demonstrated that this method of round table consideration contributes more rapidly to a solution of many problems than any procedure hitherto attempted. It was unanimously decided to follow up this meeting with a second conference to be held next fall.

NEW MEMBERS

Added to the rolls during the summer recess from May 2 to October 2, 1939.

LIFE MEMBER

McCulloch, Warren Sturgis, M.D., Neurophysiology, Yale University, New Haven, Conn.

ACTIVE MEMBERS

Adams, George F., M.A., Instructor, Geology, Coll. of the City of New York, N. Y.

Ayres, Waldemar, A.B., Market Research, International Business Machines Corp., New York, N.Y.

Belcher, Donald, Ph.D., Fellow, Chemistry, New York University, New York, N. Y.

Briwa, Kathryn E., M.A., Instructor, Chemistry, Columbia University, New York, N. Y.

Calver, Homer N., B.S., Secretary, American Museum of Health, New York, N. Y.

Cannon, A. Benson, M.D., Associate Clinical Professor, Dermatology, Columbia University, New York, N. Y.

Conner, Robert T., Ph.D., Instructor, Chemistry, Columbia University, New York, N. Y.

Cope, Otis M., M.D., Professor, Physiology and Biochemistry, N. Y. Medical College; Flower and Fifth Avenue Hospitals, New York, N. Y.

Crossley, M. L., Ph.D., Research Director, Calco Chemical Co., Inc., Plainfield, N. J.

Degering, Ed. F., Ph.D., Associate Professor, Organic Chemistry, Purdue University, West Lafayette, Indiana.

Diamond, Moses, D.D.S., Associate Professor, School of Dental and Oral Surgery, Columbia University, New York, N. Y.

Diasio, Fortunato Anthony, M.D., Dermatologist and Syphilologist, St. Elizabeth's Hospital, New York, N. Y.

Dolley, William Lee, Jr., Ph.D., Professor, Biology, University of Buffalo, N. Y.

Drogin, Isaac, Ph.D., Chief Chemist, J. M. Huber, Inc., New York, N. Y.

Dutcher, R. Adams, D.Sc., Professor, Head, Department of Agricultural and Biological Chemistry, Pennsylvania State College, State College, Pa.

Foley, George E., Medical Technician, Diagnostic Laboratory, N. Y. State Dept. of Health, New York, N. Y.

Fremont-Smith, Frank, M.D., Director, Medical Division, Josiah Macy, Jr. Foundation, New York, N. Y.

Fries, E. F. B., Ph.D., Assistant Professor, Biology, College of the City of New York, N. Y.

Gold, Harry, M.D., Assistant Professor, Pharmacology, Cornell University Medical College, New York, N. Y.

Guizot, Electra, M.A., Technician, N. Y. State Dept. of Health, New York, N. Y.

Herrick, Gerard P., LL.D., President, Herrick Vertoplane Corp., New York, N. Y.

Hinton, James William, M.D., Associate Surgeon, N. Y. Post Graduate and Bellevue Hospitals, New York, N. Y.

Jacobson, Edmund, Ph.D., M.D., Director, Laboratory for Clinical Physiology, Chicago, Ill.

Koller, Carl, M.D., Consulting Ophthalmologist, Mt. Sinai and Montefiore Hospitals, New York, N. Y.

Konheim, Beatrice G., Ph.D., Instructor, Physiology, Hunter College, New York, N. Y.

Kuhn, Harry A., M.S., Executive Officer, N. Y. Chemical Warfare Proc. Dist., New York, N. Y.

Levin, Louis, Ph.D., Research Associate, Anatomy, Columbia University, New York, N. Y.

Littman, Julius K., M.D., Instructor, Anatomy, Columbia University, New York, N. Y.

Long, Louis, Ph.D., Instructor, Pediatrics, Columbia University, New York, N. Y.

Lorand, Sandor, M.D., Graduate, Medicine, Columbia University, New York, N. Y.

Lorch, Arthur E., Ph.D., Instructor, Chemistry, Columbia University, New York, N. Y.

Lurie, Max Bernard, M.D., Assistant Professor, Experimental Pathology, Henry Phipps Institute, Philadelphia, Pa.

McNitt, Charles Wallace, M.D., Instructor, Dermatology, Vanderbilt Clinic, New York, N. Y.

Maltby, Margaret E., Ph.D., Associate Professor, Physics, Barnard College, New York, N. Y.

Mandry, Oscar Costa, M.D., CTM. (Puerto Rico), Director, Biological Laboratory, Dept. of Health; Associate, Bacteriology, School of Tropical Medicine, Puerto Rico.

Marburg, Otto, M.D., New York City, N. Y.

Marsh, M. Elizabeth, Ph.D., Assistant Director, Killian Research Laboratories, Inc., New York, N. Y.

Mavor, James Watt, Ph.D., Professor, Biology, Union College, Schenectady, N. Y.

Menkin, Valy, M.D., Instructor, Pathology, Harvard Medical School, Boston, Mass.

Meyer, Karl F., Ph.D., M.D., Director, Hooper Foundation; Professor, Bacteriology, University of California, Medical Centre, San Francisco, Calif.

Miles, Walter Richard, Ph.D., Professor, Psychology, Yale University School of Medicine, New Haven, Conn.

Milhorat, Ade Thomas, M.D., Assistant Professor, Medicine, Cornell University Medical College, New York, N. Y.

Moloy, Howard C., M.D., Assistant, Obstetrics and Gynecology, Columbia University, New York, N.Y.

Morris, Frederick K., Ph.D., Professor, Structural Geology, Massachusetts Institute of Technology, Cambridge, Mass.

Morton, Rosalie S., M.D., L.H.D., D.Sc., Surgeon, Winter Park, Florida.

Murray, Margaret R., Ph.D., Research Assistant, Surgery, Columbia University, New York, N. Y.

Myers, Chester N., Ph.D., Sc.D., Associate, Dermatology, Columbia University, New York, N. Y.

Olitsky, Peter K., M.D., Member, Rockefeller Institute, New York, N. Y.

Pallister, Helen, Ph.D., Instructor, Psychology, Barnard College, New York, N. Y.

Pearlman, William H., Columbia University, New York, N. Y.

Pierce, Harold B., Ph.D., Professor, Head, Department of Biochemistry, University of Vermont, Burlington, Vt.

Pinkston, James Oliver, Ph.D., Adjunct Professor and Chairman, Pharmacology, American University of Beirut, School of Medicine, Beirut, Syria.

Richter, Curt P., Ph.D., Associate Professor, Psychobiology, Johns Hopkins Medical School, Baltimore, Maryland.

Russell, William F., Ph.D., LL.D., Ed.D., Dean, Teachers College, Columbia University, New York, N. Y.

Salter, William Thomas, M.D., Biological Chemistry, Assistant Professor, Harvard Medical School; Associate Physician, Thorndike Memorial Laboratory; Assistant, Medicine, Boston City Hospital, Boston, Mass.

Schroeder, Charles Robbins, D.V.M., Veterinary Pathologist, New York Zoological Society, New York, N. Y.

Sells, Saul B., Ph.D., Psychologist; Director, Educational Research, Works Progress Administration, New York, N. Y.

Smallwood, Hugh M., Ph.D., Research Chemist, U. S. Rubber Company, Passaic, N. J.

Stanton, Gilman S., B.S., New York, N. Y.

Steinbach, Henry Burr, Ph.D., Assistant Professor, Zoology, Columbia University, New York, N. Y.

Steinhardt, Jacinto, Ph.D., Physical Chemist, Textile Foundation Research Associateship, National Bureau of Standards, Washington, D. C.

Sulzberger, Marion Baldur, M.D., Assistant Clinical Professor, Dermatology and Syphilology, Columbia University, New York, N. Y.

Thygeson, Philips, M.D., Oph.D., Ophthalmology, Columbia University, New York, N. Y.

Truex, Raymond C., Ph.D., Instructor, Anatomy, Columbia University, New York, N. Y.

Twiss, J. Russell, M.D., Assistant Clinical Professor, Medicine, Columbia University, New York, N. Y.

Varrelman, F. A., A.M., Professor, Biology, American University, Washington, D. C.

Vladykov, Vadim D., Ph.D., Research Associate, University of Montreal, Montreal, Canada.

Volk, Vladimar K., M.D., D.P.H., Health Commissioner, Saginaw County; Medical Director, Saginaw County Hospital, Saginaw, Mich.

von Engeln, Oskar, Ph.D., Professor, Geology, Cornell University, Ithaca, N. Y.

Warner, Marie Pichel, M.D., Jewish Memorial Hospital, New York, N. Y.

Weil, George L., M.D., New York, N. Y.

Weinbach, Ancel P., Ph.D., Instructor, Diseases Children, Columbia University, New York, N. Y.

Wheeler, John Archibald, Ph.D., Assistant Professor, Mathematical Physics, Palmer Physical Laboratory, Princeton, N. J.

Whitehorn, John C., M.D., Professor, Psychiatry, Washington University Medical School, St. Louis, Mo.

Whiteside-Hawel, Beatrice, Ph.D., Histology, Volunteer Research Worker, University of Zurich, Switzerland.

Wick, Frances G., Ph.D., D.Sc., Professor, Physics, Vassar College, Poughkeepsie, N. Y.

ASSOCIATE MEMBERS

Black, J. H., M.D., Professor, Clinical Medicine, Baylor Medical School, Dallas, Texas.

Dannecker, John M., Ph.G., Ph.C., City Chemist, New Orleans, La.

de Leon, Antonio Ignacio, Ph.D., Associate Professor, Chemistry, Agricultural College, University of the Philippines, Laguna, P. I.

Dodge, Stanley D., Ph.D., Professor, Geology, University of Michigan, Ann Arbor, Mich.

Dyer, R. Eugene, M.D., LL.D., Chief, Division of Infectious Diseases, National Institute of Health, Washington, D. C.

Eggleston, H. R., A.M., Professor, Biology, Marietta College, Marietta, Ohio.

Eyring, Henry, Ph.D., Professor, Chemistry, Princeton University, Princeton, N. J.

Fuoss, Raymond M., Ph.D., Research Laboratory, General Electric Company, Schenectady, N. Y.

Gantt, W. Horsley, M.D., Psychobiology, Phipps Psychiatric Clinic, Johns Hopkins School of Medicine, Baltimore, Md.

Greenstein, Jesse P., Ph.D., Instructor, Biochemistry, Harvard Medical School, Boston, Mass.

Hampel, C. W., Ph.D., Associate Professor, Physiology, American University of Beirut, Syria.

Heuser, Ralph V., Chemist, American Cyanamid Co., Stamford, Conn.

Hill, Samuel E., Ph.D., Lecturer, Biology, Princeton University, Princeton, N. J.

Hines, Marion, Ph.D., Associate Professor Anatomy, Johns Hopkins Medical School, Baltimore, Md.

Hitchcock, David I., Ph.D., Associate Professor, Physiology, Yale University, School of Medicine, New Haven, Conn.

Jensen, H., Ph.D., Institute of Experimental Biology, University of California, Berkeley, Calif.

Kilpatrick, Martin, Ph.D., Associate Professor, Chemistry, University of Pennsylvania, Philadelphia, Pa.

Kimball, Arthur Livingstone, M.E.E., Consulting Mechanical Engineer, General Electric Company, Schenectady, N. Y.

Kirk, William, Jr., B.S., Graduate in Organic Chemistry, Cornell University, Ithaca, N. Y.

Kirkwood, J. G., Ph.D., Professor, Chemistry, Cornell University, Ithaca, N. Y.

McCullagh, D. Roy, M.D., Cleveland Clinic, Cleveland, Ohio.

Maloney, A. H., M.D., Ph.D., LL.D., Professor, and Head, Department of Pharmacology, School of Medicine, Howard University, Washington, D. C.

Medlar, Edgar M., M.D., Director, Hegeman Memorial Research Laboratory, Mt. McGregor, N. Y.

Miller, Richard G., Instructor, Daycroft School, Stamford, Conn.

Mills, Clarence A., Ph.D., M.D., Professor, Experimental Medicine, University of Cincinnati, Ohio.

Mueller, Hans, Sc.D., Associate Professor, Physics, Massachusetts Institute of Technology, Cambridge, Mass.

Nichols, Robert L., A.M., Geology, Tufts College, Medford, Mass.

Onsager, Lars, Ch.E., Ph.D., Assistant Professor, Sterling Chemistry Laboratory, Yale University, New Haven, Conn.

Owen, Benton Brooks, Ph.D., Assistant Professor, Sterling Chemistry Laboratory, Yale University, New Haven, Conn.

Schaeffer, Hugh C., B.S., Supervisor, United Geophysical Co., Pasadena, Calif.

Schafer, Paul A., Balatoc Mining Company, Bagnio, P. I.

Schouten, F. H., B.A., District Geologist, Stanolind Oil and Gas Company, Midland, Texas.

Schultz, Charles Wesley, B.S., Chemist, Baldwin, N. Y.

Selye, Hans, Ph.D., Assistant Professor, Histology, McGill University, Montreal, Canada.

Serviss, Frederick L., E.M., M.S., Professor, Geology, Purdue University, Lafayette, Ind.

Straley, H. W. III, Ph.D., Department of Geology, Baylor University, Waco, Texas.

Van Tuyl, Mary C., Ph.D., Research and Teaching Fellow, Psychology, University of Michigan, Ann Arbor, Mich.

van Wert, Leland Russell, Sc.D., Research Engineer, The Leeds and Northrup Co., Philadelphia, Pa.

Ver Steeg, Karl, Ph.D., Professor, Geology, College of Wooster, Wooster, Ohio.

Walker, Burnham S., Ph.D. M.D., Professor, Biochemistry, Boston University, School of Medicine, Boston, Mass.

Wanless, Harold R., Ph.D., Associate Professor, Geology, University of Illinois, Urbana, Ill.

West, D. Evelyn, B.S., Chief Microbiologist, Bureau of Laboratories, State Dep't of Health, Hartford, Conn.

White, Elizabeth J. G., Ph.D., Bennett Junior College, Milbrook, N. Y.

White, Paul D., M.D., Massachusetts General Hospital, Boston, Mass.

Wieland, G. R., Ph.D., Paleontological Research, Osborn Botanical Laboratory, Yale University, New Haven, Conn.

Wilhelmj, C. M., M.D., Professor, Physiology, Creighton University, Omaha, Neb.

Wilkens, George A., Ph.D., Chemical Engineer, E. I. duPont de Nemours & Co., Arlington, N. J.

Williams, George D., Ph.D., M.D., Assistant Professor, Anatomy, Washington University of Medicine, St. Louis, Mo.

Williams, Llewelyn, Botanist, Ministry of Agriculture, Caracas, Venezuela.

Williams, Robert G., B.S., Biology, Scranton, Pa.

Wilson, C. P., B.S., Chemist, Manager, Research Department, California Fruit Growers Exchange, Ontario, Calif.

Wood, David A., M.D., Associate Professor, Pathology, Stanford University, School of Medicine, Calif.

Young, H. A., Ph.D., Assistant Professor, Chemistry, College of Agriculture, University of California, Davis, Calif.

Zingg, Robert M., Ph.D., Anthropology, University of Denver, Colo.

STUDENT MEMBERS

Bacon, Robert L., Biology, Hamilton College, Clinton, N. Y.

Hershkowitz, Aaron, M.S., Biology, New York, N. Y.

Van Alstine, Ralph Erskine, M.S., Geology, Princeton University, Princeton, N. J.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 | December, 1939 | No. 2

SECTION OF GEOLOGY AND MINERALOGY

November 6, 1939

Doctor John B. Mertie, Jr., Senior Geologist, U. S. Geological Survey, Washington, D. C.: *Geologic Features of Alaska.* (This lecture was illustrated by lantern slides.)

Geologic mapping in Alaska began in 1896, but it was not until 1903 that a separate organization devoted exclusively to Alaska's work was established in the U. S. Geological Survey. Up to the end of June, 1938, when the last estimate was made, 258,536 square miles had been mapped on different scales, of which about 42 per cent had been done on a scale of 1:500,000, 57 per cent on a scale of 1:250,000, and less than 2 per cent on a scale of 1:62,500 or greater. More than 325,000 square miles, or 56 per cent of the Territory still remains geologically unmapped.

Alaska, because of its diastrophic history, probably presents a greater proportion of complex geology than do the United States as a whole and this fact, in conjunction with the character of the mapping so far done, makes it necessary to emphasize that the present status of geologic mapping cannot possibly be compared, either in quantity or quality, with what

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 2, No. 2, December, 1939.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

has so far been done in the rest of the United States. Hence no man can speak authoritatively and in detail of the geologic history of Alaska, but, on the other hand, many geologic facts of a general nature are known that are worthy of statement.

Alaska is nearly everywhere either mountainous or hilly, the only areas of low relief being in the deltaic portions of the large rivers, the Arctic coastal plain, and certain down-warped areas. The principal mountain range of the Territory is the Alaska Range, which sweeps in a crescentic arc across southern Alaska and disappears in the Alaska Peninsula. Another but lower range of mountains, called the Brooks Range, crosses northern Alaska. The great intermontane province known as interior Alaska is characterized by isolated groups and short ranges of lower mountains separated by great expanses of rolling hills. This province is drained by the Yukon and Kuskokwim Rivers, the two largest streams of Alaska. The Alaska Range swings far north of the shores of the Pacific, thereby creating the province of southern Alaska which is characterized by various coastal ranges separated by lower hills. North of the Brooks Range is the province of northern Alaska, characterized by rolling hills bounded on the extreme north by the Arctic coastal plain. The panhandle, known as southeastern Alaska, constitutes another geographic and geologic province. Finally, at the west and southwest sides of Alaska, there are areas like Seward Peninsula and the Alaska Peninsula, together with its continuation in the Aleutian Islands, which should properly be regarded as distinct from any of the provinces heretofore mentioned.

Geologic formations of all ages are represented, but no complete sequence is present in any one of the several provinces. In general, the pre-Cambrian rocks are best exposed in interior Alaska, though such rocks also occur in Seward Peninsula and possibly in southeastern Alaska. The Paleozoic sequence, as a whole, is best developed in interior Alaska and in the Brooks Range, though such rocks likewise are present in Seward Peninsula and southeastern Alaska. In southern Alaska, few

Devonian or older rocks are known, but this province shows a complete sequence of Carboniferous rocks and many formations of Mesozoic age. The Carboniferous rocks and a considerable part of the Mesozoic sequence are likewise well developed in northern Alaska. The Alaska Peninsula is characterized by the presence of a complete Mesozoic sequence. Here, in fact, is the standard marine Jurassic section of North America. The western end of the Alaska Peninsula and the Aleutian Islands are the sites of great lava fields of Tertiary and Quaternary ages. Sedimentary rocks of Tertiary age are present in many parts of Alaska but nowhere form any considerable part of the geologic sequence. Southeastern Alaska, rather isolated from the rest of the Territory, stands out as the site of the granitic rocks of the Coast Range, though it also shows a fine sequence of Paleozic rocks and a lesser sequence of Mesozoic rocks.

The history of diastrophism and mountain building is far from complete. The major discontinuities in sedimentation and structural unconformities of pre-Cambrian and Paleozoic time have been recognized, but their true meaning, particularly in terms of mountain building, is still obscure. The present mountains originated in the Mesozoic and Cenozoic eras. Some of the southern and southeastern coastal ranges are believed to have originated mainly as a result of epeirogenic uplift at the end of the Jurassic period followed by differential erosion, though both earlier and later deformation and uplift of these mountain masses has been recognized. Invasion of the sea in Lower Cretaceous time was followed in the mid-Cretaceous by differential uplift and deformation of the coastal region, which, in turn, was succeeded by the formation of a great thickness of Upper Cretaceous marine sediments, particularly in southern Alaska. In the Upper Cretaceous epoch, however, terrigenous sediments were also deposited at the present site of the Alaska Range. It is believed, therefore, that the differential uplift of mid-Cretaceous time exerted its maximum effect north of the coastal ranges, and, in fact,

accentuated the highland area that already existed at the site of the present Alaska Range. The succeeding stratigraphic and diastrophic events in southern Alaska were approximately as follows:

1. Epeirogenic uplift at the end of the Cretaceous period.
2. The deposition in the Eocene epoch of terrigenous deposits containing coal.
3. The differential deformation of the coal measures during the Oligocene epoch resulting in down-warping and preservation of one part and up-warping and erosion of another part.
4. Pronounced differential uplift and deformation in the Miocene epoch resulting in the formation of the Nenana gravels and further uplift of the Alaska Range.
5. Epeirogenic uplift in the Pliocene epoch and further mountain building by strike faulting and differential erosion. The St. Elias Range and other coastal ranges may have been further elevated at this time.
6. Glaciation and local uplift in the Pleistocene epoch.
7. Vanishing glaciation in Recent time accompanied or followed by a general elevation of the base level of erosion.

Glaciation is one of the more spectacular geologic features of Alaska. At the time of maximum glaciation, all of southern and southeastern Alaska was covered by a vast ice-cap, which extended at places for 100 miles or more beyond the limits of the present shore line in much the same manner as the present Antarctic ice sheet. Another, but much smaller ice-cap, covered the Brooks Range of northern Alaska but did not extend to the Arctic Ocean. These two ice fields coalesced, however, in the upper Yukon Valley of Canada, yet interior Alaska, except for a few small areas, remained entirely unglaciated throughout the Ice Age. A third, but comparatively small ice-cap, which did not connect with the ice fields of southern Alaska, also existed north of Bristol Bay. In Alaska, the term Pleistocene corresponds only roughly with the Ice Age, for glaciation may have started earlier and certainly persisted longer than in the northern United States. At the present

time, several large ice fields still remain in southern Alaska, though together they constitute only about 4 per cent of the area of the Territory. The several glacial and interglacial stages of the Pleistocene epoch that have been established in the northern United States have not been recognized in Alaska, partly because the more intensive studies of glaciation have been made in southern Alaska, between the Alaska Range and the Pacific Ocean, where the intense Wisconsin glaciation has largely obliterated the evidence of any earlier glacial stages. Some cogent evidence regarding pre-Wisconsin glaciation, however, has been adduced.

The distribution and discovery of mineral deposits in Alaska has been materially influenced by glaciation. No *a priori* reason is known why lode and placer deposits should not have coexisted north and south of the Alaska Range before the beginning of the Ice Age. Yet, at the present time, nearly all of the important placers are restricted to interior Alaska and Seward Peninsula, whereas most of the important lodes have been found in southern and southeastern Alaska. The distribution of the placers is easily explained because glacial action in the Alaska Range and coastal ranges and seaward therefrom was so severe that the country rock was largely denuded, thus eroding and dissipating most of the pre-Pleistocene alluvial deposits. The meaning of the present distribution of lode mines in Alaska is less apparent, but it is possible that the denudation of bed-rock by glaciation has been an important factor favoring the discovery of lodes in southern and southeastern Alaska. On the other hand, the heavy cover of residual and alluvial deposits has certainly been a handicap to lode prospecting north of the Alaska Range and may be an important factor in their apparent scarcity in that region.

SECTION OF BIOLOGY

NOVEMBER 13, 1939

PROFESSOR A. E. PARR, Director, Peabody Museum, Yale University, New Haven, Conn.: *On the Functions of the Natural History Museum.*

The natural history museum of today is rather on the defensive. It can no longer claim justification by the mere existence of its collections. After a period of indiscriminate expansion which has been virtually unbroken since its early beginnings, the museum has entered into an era of consolidation and evaluation of its functions. It is therefore of urgent necessity to attempt to find a clear-cut expression for the services which a museum of natural history is capable of rendering and for the need of society to have these services performed.

It has been clearly brought out by the President of the American Museum of Natural History that the modern museum has to seek its support from an everwidening segment of the population and can no longer be fully maintained by the generosity of the few. This is a fact of great significance. It means that the museum can no longer devote its efforts entirely to the satisfaction of the abstract scientific curiosity for which a full appreciation can only be found within a comparatively small group of enthusiasts in all walks of life. Instead, an increasing emphasis must be placed upon the treatment of the more concrete problems in man's relation to nature which are of common concern to all mankind.

The possible functions of the natural history museum can be divided into four categories. At one extreme, we have (1) its functions for or in research. At the other (2), its pure entertainment value. In between, we have (3) the museum's services in support of formal education in other educational institutions, and (4) its maintenance of an independent educational program of its own.

In relation to research the museum of natural history has a dual set of functions to perform. It serves as an archive for the preservation of the evidence of conclusions already arrived at. It is also expected to form the center of active research towards new conclusions. These dual purposes are not in harmony but in competition, each being a burden upon the other. In this internal conflict there is grave danger to the effective functioning of the museum.

The archive function can best be analyzed in relation to the so-called type specimens. The value of type material is not absolute and permanent, but temporary and relative. During the period between the first discovery and the attainment of a firmly established knowledge concerning a new kind of natural history object the preservation of the examples upon which the first description was based is highly essential. Beyond this period it loses its special significance. Whether or not the type specimen of the North Sea Haddock is still available is no longer of any actual importance to science.

To the extent that the maintenance of type material represents a real scientific need it is obvious that this need will be better served the more complete we make each natural unit of type specimens gathered under one single roof. In view of this fact, the current practice of jealously retaining any type specimen, however odd and isolated, in whichever museum first happens to gain possession of it, expresses a total disregard for the best interests of science and must be condemned as an archaic method of procedure. Through this chaotic state of disorganization the archive function of the museums of natural history is getting closer to a complete breakdown every day, and little credit is due for the manner in which this function is being performed at the present time. It is therefore urgently required that a voluntary system of division of labor in the maintenance of type material should be developed between the major museums which are adequately equipped to take their share of responsibility for the care of research collections of this sort. Under such a system each primary museum might

be designated as a repository with definite responsibilities for definite systematic groups of natural history objects, receiving in its care all type material in these groups, regardless of where collected.* It could of course easily be arranged for minor museums, mainly concerned with educational functions, to receive material of greater value for their purposes in return for such scientific type material as they might agree to surrender. Unless some such plan of organization is put into effect, an increasing disregard for the practice of comparison with type material must unavoidably develop, spelling the bankruptcy of our archive functions.

Intimately connected with the archive function of the museum is its oldest function in active research, namely, that concerned with the systematic classification of natural history objects. Since a major part of my own work has been along these lines, I hope I may be permitted to say that one of the difficulties about maintaining the scientific prestige of a natural history museum derives from the fact that this type of research, however well done, has lost the high place it once occupied in public and scientific esteem. This is, in part, entirely unjust. Without a continuously expanding and improving system of classification, all search for new things to discover must soon come to an end, since it is only through classification that knowledge actually becomes available to mankind and can be used in relation to other knowledge for the advancement of human interests. The search for new discoveries and the development of their classification are therefore inseparable parts of human progress, and, since the unknown cannot be evaluated, it is impossible in advance to orient our search for new knowledge on the basis of its ultimate value in the affairs of man. It is therefore essential for human progress that there should be institutions devoted to the accumulation of pure knowledge without any regard for immediate usefulness. In

* Through its effects upon staff appointments such a system would also help towards securing an evenly distributed availability of technical advice on *all* systematic groups.

the field of natural history, this has come to be peculiarly the function of natural history museums through the search for, and study of, their collections. From the richness of their apparently "useless" knowledge, the museum types of systematic research endow every new branch of "useful" applied science with a heritage of classified and systematized information without which the exploitation of the useful facts could not find its starting point. But, granting that any criticism of the ideal value of systematic research is entirely without foundation, one can scarcely deny that there is a very considerable reason for criticism of systematic practices. Taking as an example that particular branch of systematic research called taxonomy, I believe it is a common and grievous error to regard the system not only as a means to an end but as an end in itself. It might be well to remember that the broad basis on which our taxonomy still rests was introduced by Linnaeus long before any theory of evolution. The main purpose of taxonomy should be to develop the most convenient, most precise, and most unequivocal methods of classifying human knowledge so that confusion may be avoided and comparison made. Taxonomy has fallen rather miserably short of this purpose, largely due to the fact that it has substituted for common sense a rigid system of legalistic rules of procedure for the protection of personal vanities. It was a prominent feature of the original plans for a systematic classification of nature that stability of designations should be assured by a nomenclature based upon dead languages no longer subject to change. Because no personal ambitions are involved, it has come to be easier to achieve a stabilization of vernacular names than of scientific terminology, and modern taxonomic nomenclature is scarcely more stable than modern slang.

Since it seems indicated that taxonomy has created rather than encountered its own main difficulties, it is also indicated that those difficulties can be overcome. But even when this is accomplished, it seems very unlikely that taxonomy alone will ever again suffice for the maintenance of the museum's stand-

ing in research, and for the justification of the cost of its research activities and its research collections.

In the problem of determining the research functions which will provide the fullest and best utilization of all its facilities, it may be well to consider the totality of the museum as though it were a single instrument. On this basis we may say that, just as the proper functions of a microscope are determined by its ability to magnify very small objects, so also may the proper research functions of the entire museum be determined by its special ability to handle natural history objects in large quantities. Since this ability, peculiar to the museum as a whole, fulfills one of the primary needs peculiar to the statistical fields of research, such as ecology and population studies in general, it is logical to see in the pursuit of this type of investigations the primary research function of the natural history museum. In a vast field of ecological problems, preserved samples offer the same advantages over the attempts to observe living nature as those found in a series of stills culled from a motion picture film covering action too complex to be analyzed in movement. Preserved samples also offer other great advantages in the concentration of evidence and reduction of dimensions in space, which are essential for the practical undertaking of ecological research. Of course, if we do recognize that one of the main research missions of a natural history museum lies in the ecological types of investigation, we must obviously also admit the perfect appropriateness of handling live, as well as dead material, in the museum to the extent that the observations on the living individuals may tend to confirm or to explain the conclusions arrived at by the study of the preserved samples of natural populations.

That ecological studies do not provide for such a rapid accumulation of new or rare species in museum collections is a commonly heard but scarcely valid objection. In a realistic appraisal of the natural significance of things, it is evident that the rarest object is as a general rule also the least important one by the very reason of its rareness. There are furthermore

far too many "unique" specimens known today, and there is far too little faith in their actual uniqueness to warrant the maintenance of museums for the acquisition and preservation of such curiosities as their main purpose. If we wish to use the capacity of the museum to handle natural history objects in quantity to the greatest benefit for scientific progress by applying the facilities of the museum to the lines of research in which this capacity is most particularly needed, we must indeed give as much attention to the rational disposal of collections as to their acquisition, so as to insure the continued availability of the capacity on which the museum's usefulness depends. We must adopt the attitude that beyond the actual needed reference collections, and the material for which the museum may have been designated as a repository, all other collections will have to be treated as strictly temporary, to be discarded when they have served the purpose for which they were obtained.

Perhaps we may also soon begin to look forward to the time when even the need for permanent reference collections may be substantially reduced by an adequately organized system of corresponding institutions and individuals in all parts of the world, each agreeing to supply on request fresh material of such natural history objects as are easily obtainable within its region. If by some reliable system, we can obtain a sample on reasonable notice when the need actually arises, it is obviously far more economical in every way to leave the Haddock on Georges Bank until wanted, than to keep it always available in preservation in anticipation of a possible demand which may never materialize until our specimens have gone bad anyway.

In regard to the museum's place among the so-called "entertainment industries," I do not see anything objectionable about frankly acknowledging that we do not only teach but also simply entertain. It is hardly necessary to point out that in combining these two functions we are in the most excellent and highly respected company. Our type of entertainment is sound and, when well done, of considerable intrinsic

value to the human imagination, perhaps particularly to the developing minds of the young. We should be perfectly entitled to take pride in this fact and to expect society to take some interest in our activities for their genuine entertainment value alone, even if no other services were offered.

But the museum has merits and responsibilities beyond mere entertainment. Prominent among its duties is the obligation to make its exhibits serve the needs and supplement the shortcomings of formal education as given in the classrooms. In the classroom, the study of nature is, of practical necessity, artificially subdivided into a number of separate subjects of instruction, such as geology, botany, zoology, etc. It seems to be generally assumed that the museum's main function in relation to this departmentalized classroom teaching is one of maintaining a corresponding set of departmentalized exhibits for the use of the educators in each separate subject. In consequence, one usually finds the organization of the museum of natural history developed along the same lines as the organization of classroom teaching. That the availability of the traditional exhibits, artificially segregated by subject, is of great importance to formal instruction is undeniable, but whether they constitute the most significant and valuable contribution the museum can make to systematized education is highly questionable. In classroom teaching, the natural phenomena are not divided according to their functional relationship in the totality of contemporary nature, but according to the historical relations of their past descent. This classification of studies is obviously of great pedagogical advantage as a means of reducing the amount of data which have to be conveyed to the student. It permits each phenomenon to be described as a derivation and modification of another, whether by organic evolution, inorganic metamorphosis, or simply by mental manipulation, thereby making the acquisition of knowledge cumulative and reducing repetitive information to a minimum. But even while we arrange our instruction entirely according to historical origins, we are fully aware of the fact that nature itself

is rather supremely indifferent about the past history of the living and dead objects it fits together to form a functional unit in the present with which humanity is, after all, chiefly concerned. In dynamical language one might say that the evolution of nature is so slow that its functional relations may be treated as a series of equilibrium states, which at any given time will find their full explanation in the contemporary conditions alone, without consideration of historical factors. The price which formal education pays for the advantages offered by the purely historical classification of natural history subjects therefore tends to be one of a badly distorted perspective in the impression it gives of nature as a whole. In the museum, all branches of the natural history subjects are brought together under one roof. Due simply to its larger cubic space, the museum also offers an opportunity to demonstrate the interrelations between phenomena where the classroom is confined to a showing of individual objects. The most valuable contribution the museum can make to further the true goals of all formal education is probably to be found in a full utilization of these special opportunities in an attempt to create a picture and philosophy of nature as a whole, supplementing rather than merely supporting the classroom instruction, and providing a correction for the distortion of perspective arising from the artificial subdivision and segregation of the teaching subjects.

If we change the intellectual orientation of the museum program from a concentration upon the diversity of natural objects to an emphasis upon the unity of nature as a whole, it would also become desirable to modify the customary patterns of human organization and architectural arrangements in a corresponding manner. Curatorial departments should be formed in relation to various types of environment, each with its own characteristic mechanisms through which the unity of nature finds its expression. One might have superdepartments of terrestrial and of aquatic environments, of historical biology and geology. One might have further subdivisions into polar,

temperate, and tropical subdepartments, or departments for tropical forest, desert, swamps, etc. In each such department, one would be concerned with the totality of all natural manifestations, whether living or dead, which form the characteristic features of the particular type of environment to the study and exposition of which the department is dedicated. Under such a system, the classification and identification of individual objects and the care of systematic reference collections would be organized as a separate and independently integrated service available to all departments. In practice, this would not involve two entirely separate staffs but rather dual appointments for many individuals, such as, for instance, "curator of oceanography and ichthyologist," "curator of tropical forest life and chief botanist." In his curatorial capacity, a staff member would be in charge of a department, reporting only to the chief curator. In his other capacity, his special systematic knowledge would be made available to the entire museum through an organization headed by the chief systematist. Only in the larger museums would the staff come to contain many members who were either exclusively systematic consultants or exclusively concerned with the functions of a curatorial department.

In the architectural arrangements, the shift in emphasis would mean that exhibits demonstrating the unity of nature would have to be given the central and most effective space, with the diversity of the various systematic groups of phenomena displayed as lateral digressions from the theme of the main halls. The subjects of natural history may be said to form a pattern consisting of two geometrical figures. The open progression of a straight line representative of the historical derivation of the form and properties of the individual objects, and a closed circle representative of the interrelationships between all coexistent objects at any given time. This abstract pattern lends itself very well to architectural expression in a U-shaped central arrangement of three main exhibition halls, from which lateral halls or alcoves would radiate in

all directions. The visitor would enter the museum at the narrow end of a long hall dedicated to a quasihistorical presentation of the organization of nature. Some attempt would be made to illustrate the structure of matter and the behavior of its elementary components. A selected exhibit of naturally occurring pure elements and of the isolated pure compounds of such elements, which we call minerals, would follow, with an exposition of their manner of formation and transformation. From mineralogy, we would proceed to the formation, composition, and metamorphosis of rocks. At this point, an archway would open into a lateral hall, showing in greater detail the diversity of minerals and rocks and of the processes affecting them. Having surveyed the materials of the earth, one would turn to a consideration of the geophysical forces acting with and upon these substances, the mechanisms by which they operate, and the results which they produce. Another archway would open to a lateral hall of general geology and geophysics. Our next step would carry us to the simplest and most primitive manifestations of life, and, continuing down to the end of the hall, we would finally come to man's place at the end of the sequence.

The visitor would then enter a large square or semicircular hall at the bottom of the U, dedicated to demonstrations of the closed cycles of functional interrelationships in nature, from such simple and elementary interdependencies as that between plants and animals with regard to metabolism of gases and to nutrition, to the most complex cycles in which living nature and geophysical factors interact in a complete and unbroken system. The alcoves radiating from this hall would be devoted to a more detailed treatment of the cycles and ecological mechanisms of the main categories of environment, such as marine, fresh water, and the various types of terrestrial habitats.

Leaving again, the visitor would walk back towards the main entrance through a second long hall forming the other limb of the U, parallel with the hall he first entered. In this

hall he would be shown in logical seqence how the natural processes and relationships explained in the preceding halls affect the affairs of man, the possibilities they offer, and the limitations they set to human efforts, and how these efforts affect the balance of undisturbed nature. There would be lateral halls elaborating upon such subjects as agrobiology, forestry and wild life, fisheries biology, soil hydrology and chemistry, applied geophysics, etc.

In a museum arranged in some such fashion as that just outlined, every visitor would perforce be exposed to the educational effects of a continuous arrangement of exhibits, telling a balanced and coherent story of nature as an entity in itself and as the environment of man, while the extent to which the spectator might want to deepen his knowledge of details in any particular category of natural phenomena would be entirely optional, dependent upon whether or not he chose to enter any of the lateral halls. By keeping the totality of nature in all its aspects always before the visitor and not permitting special knowledge to be obtained from its displays, except in proper perspective to the whole, the museum would be doing far more to give real contents to formal education in any particular branch of the natural history subjects than by concentrating its efforts upon a further elaboration of details in these branches separately.

There finally remains to be considered the museum's own educational program, independent of the classroom or lecture room. It seems evident that all forms of education should in the ultimate analysis be directed towards the attainment of a successfully functioning society. It is therefore not enough to design an educational program simply on the basis of the abstract philosophical structure of knowledge. The needs of the citizen for the various types of information contained in the sciences vary according to the responsibilities placed upon him by the form of society of which he is a member. We are fortunate enough to enjoy the advantages of the democratic form of government and it should be the duty of the museum,

no less than that of all other educational institutions, to design its program in relation to this fact to the end that the democratic system may be made to function ever better.

It is characteristic of the democratic form of government that all decisions are in the ultimate analysis made by laymen, either by the direct decision of his vote or indirectly by his judgment upon the performance of those he elects to make decisions for him. For the purpose of our discussion, we may divide the responsibilities which thus fall upon every citizen of a democratic country, regardless of his knowledge, into two categories. In the first category, we may put his responsibilities for all decisions involving the care for man himself. That is, the administration of personal health, community health, the moral and legal relations between men living together in civilized society, and so on, and so forth. On all subjects of this sort, our educational system provides the layman with some minimum of information. But when we turn to the second category of democratic citizenship responsibilities, namely the responsibilities which arise from the participation in decisions involving the care for the natural environment of man as distinct from man himself, the care for the country as distinct from the care for the nation, we find that our general educational system usually fails entirely to give even the slightest suggestion of the type of knowledge and reasoning on which such decisions should be made. Although the layman majority decides, even the most elementary knowledge of principles is carefully reserved for the training of the professional minority. Each community decides its own local problems; each state passes its own regulations on questions of wider scope; and the federal government exercises its supreme power of control when problems of a national character arise. All of these decisions, however general or specific, are largely formed under the pressure of public opinion, often through the direct expression of this opinion by the method of the vote. Yet it is generally quite evident that this is not an informed opinion, guided by the independent reasoning of the

individual on the basis of such elementary knowledge as society might logically have been expected to provide him with while preparing him, through education, for his future participation in the government of the land as well as the government of man. Every one of us who has had any kind of contact with applied biology or applied geophysics is familiar with some of the chaotic regulations which are so often put into effect and some of the highly needed measures which could not be taken due to the lack of public understanding which arises from this educational neglect.

Here again the natural history museum has an opportunity to make its usefulness particularly strongly felt, especially if it has already adopted the policy of making the unity rather than the diversity of nature the main theme of its educational efforts. Just as nature refuses to recognize any barriers between various categories of natural phenomena, so also do the problems arising from man's relations to nature break all artificial barriers between abstract disciplines of knowledge. The question of deforestation or reforestation is not a botanical problem, nor a simple problem of agricultural expansion or contraction, but one which involves botany, hydrology, soil geology, climatology, animal biology, agriculture, and national economy. If the contention is valid that the museum is better adapted than the classroom to deal with the comprehensive aspects of nature, it also seems obvious that it is incumbent upon the museum to recognize a particular responsibility towards general public education in the principles governing man's dependency upon the substances and forces of his natural environment. In fulfilling this responsibility, the museum should give careful consideration, both to the needs for information arising from the special circumstances peculiar to the local community and environment and to those related to current or permanent problems in the national affairs upon which the local community also exerts its proportionate influence through the state or federal type of organization.

In relation to national questions, the metropolitan museums

have a particular duty to perform in presenting the problems of land and sea to a community which is otherwise without direct contact with the natural foundations of human existence, but which, nevertheless, by democratic procedures and also by financial strength, exert a very potent influence upon the manner in which these problems are dealt with by the nation as a whole. There should, for instance, during recent years have been a considerable obligation to present without prejudice such knowledge and such theories as have come under consideration in the national approach to the questions of drought and flood control.

The adjustment of the museum program to the natural interests of the local environment can perhaps be illustrated by an abstract comparison of the treatment one would want to accord the subject of fisheries biology in the metropolitan museum and in a museum located in a smaller community directly dependent upon the harvests of the sea for its main livelihood. For the inhabitant of the large city, the immediate impact upon his own life of the short-range and localized fluctuations in the abundance and character of nature is slight. His comforts depend upon average, not upon specific, yields. His concern is with the long-term trends and general variations. To the direct dependent upon the harvests of nature in any form the long-term trends and general aspects are only of secondary significance. If his particular locality in any particular year produces an abundance, his comforts are secured and he is well off for another year, regardless of whether general trend in larger units of space and time is sharply downward or upward. His bet is placed upon the violent short-term and localized fluctuations of nature. Even within the treatment of the same subject there should thus be a complete reversal of emphasis from the metropolitan museum to the museum of the fishing community. In the latter, one would stress the causes and character of the violent annual fluctuations to which any fishery is subject. Dangerous or favorable long-term trends in abundance would be given secondary consideration, with an exposi-

tion of their causes and cures so far as known. Exhibits devoted to geographic comparisons and to the causes of the permanent geographic differences in the character and abundance of marine life would be regarded as definitely subordinate to the other two categories of displays. In the metropolitan museum, geographic comparisons would be first; long-term trends, second; and short-period fluctuations third in importance.

Whether the various suggestions made in the preceding are acceptable or not, it seems evident that the natural history museum has reached a stage in the evolution of its relationship to society where the generally prevailing opportunistic vagueness of intentions is becoming a liability, which must be replaced by a well-considered, well-integrated, and well-defined philosophy concerning the museum's place in the general research and educational system of the nation, working towards the common goal of an ever better management of human affairs by democratic methods of government.

SECTION OF PSYCHOLOGY

NOVEMBER 20, 1939

DR. GEORGE KATONA, New School for Social Research, New York City: *Organization in Human Learning.*

The author reported on a research work the goal of which was to determine whether it is possible to differentiate between two kinds of learning, *mechanical memorizing* and *learning by understanding.* This differentiation appears to be justified only if the results of these methods of learning differ significantly from each other. Experiments were therefore performed in which the same material, *e.g.,* the mastery of a difficult new problem, was taught in several ways and the results of learning under each of these methods were compared.

One extreme method of teaching consisted in letting the subjects (college undergraduates) memorize by frequent repetitions the solution of a problem. In other experiments, the subjects memorized certain steps leading to the solution without reference to the role and function of the steps within the whole process. These methods may be called learning by drill or by forming and strengthening arbitrary connections. The other extreme method of teaching consisted in giving certain carefully chosen cues or helps to the subjects, who, with the aid of these cues, were able to solve the problem by understanding the transformation of a first situation (representing the unsolved task) into a later situation (representing the solution of the task). The various learning processes took the same time, were demonstrated on the same tasks, and were made comparable in various ways.

In the quantitative experiments, one result of the learning processes was studied primarily, namely,—the ability of the learners to apply under slightly or greatly changed circumstances the knowledge acquired in the training period. It was found, in agreement with well established data, that in most instances mechanical memorizing yields only an inconsiderable

transfer effect. Different results were, however, obtained by those subjects who learned by understanding. In delayed tests, those specific tasks, which were seen in the practice period, were not solved more readily than tasks which were not seen before but which made use of the same principle. The subjects who had learned by understanding were unable to reproduce the exact content or form of the practiced tasks a few weeks after the training period. What they knew at that time—often without being able to formulate it in words—were certain structural principles, representing qualities of the process and being more or less independent of the specific material which, some weeks earlier, served as the learning material. The subjects did not carry over a performance or a bit of knowledge from the learning material to a new material, but applied their integrated knowledge both to the practiced and the unpracticed tasks. They achieved about the same success in both cases.

Learning by understanding may therefore be differentiated from mechanical memorizing. The former appears to consist of an organization or a reorganization in such a way as required by the material or the task. Reproduction as the consequence of learning by understanding has the form of reconstruction. The individual contents are reconstructed on the basis of "whole-qualities" which alone are remembered. When whole-qualities are better preserved than the specific items, we can apply the former under changed circumstances,—which thesis may serve to explain the extensive transfer effect found in the experiments.

Organizing is, however, not a quality characterizing the process of learning by understanding alone. Even in mechanical memorizing we can recognize rudimentary organizing (or grouping) processes. But, in some cases, organization does not bring about understanding, because the learner is unable to achieve an adequate organization, or because the learning material makes adequate organization impossible (*e.g.*, the latter is the case in rote learning of nonsense syllables).

The investigation, of which a brief summary is given here, was strongly influenced by the "gestalt theory" as developed by Professor Max Wertheimer. Acknowledgement is made to the Carnegie Corporation of New York for a grant-in-aid. The study reported will constitute a part of the author's forthcoming book on "Organizing and Memorizing," to be published by the Columbia University Press.

SECTION ON PHYSICS AND CHEMISTRY

November 10 and 11, 1939

Conference on *"Free Organic Radicals as Intermediate Steps in Oxidation."*

The Section of Physics and Chemistry held a Conference on "Free Organic Radicals as Intermediate Steps in Oxidation," the first of the series this year. Doctor L. Michaelis, Rockefeller Institute for Medical Research, was in charge of this meeting as Conference Chairman.

The program consisted of the following papers:—

"Occurrence and Significance of Semiquinone Radicals," by Doctor L. Michaelis, Rockefeller Institute for Medical Research.

"The Quantum Mechanical Aspect of the Stability of Radicals," by Doctor G. W. Wheland, University of Chicago.

"Application of the Dropping Mercury Electrode for the Detection of Intermediate Radicals," by Doctor O. H. Müller, Cornell University Medical School.

"The Analogy Between Two Step Oxidation and Two Step Ionization," by Doctor M. P. Schubert, Rockefeller Institute for Medical Research.

It is planned to hold two additional conferences in this series, announcements of which will be made later.

NEW MEMBERS

Elected November 6, 1939

SUSTAINING MEMBER

Stackpole, Caroline E., M.A., Associate, Biology, Teachers College, Columbia University, New York City.

ACTIVE MEMBERS

Craig, Gerald S., Ph.D., Associate Professor, Natural Science, Teachers College, Columbia University, New York, N. Y.

Davidson, Morris, M.D., Ophthalmologist, N. Y. State Dept. of Labor, New York, N. Y.

Deere, Emil O., A.M., S.M., Professor, Biology and Geology; Dean, Bethany College, Lindsborg, Kansas.

Failla, Gioacchino, E.E., D.Sc., Professor, Physicist, Memorial Hospital, New York, N. Y.

Frank, Richard L., M.D., Instructor, Psychiatry, Columbia University, New York, N. Y.

Gardner, William A., M.D., Associate Medicine, Columbia University, New York, N. Y.

Hart, Fanchon, A.M., Professor, Bacteriology, Columbia University, New York, N. Y.

Heiman, Jacob, M.D., Associate, Cancer Research, Columbia University, New York, N. Y.

Hixson, A. W., Ph.D., Acting Executive Officer, Chemical Engineering Dept., Columbia University, New York, N. Y.

Holland, Margaret, B.S., Instructor, Physical Education, Barnard College, Columbia University, New York, N. Y.

Holliday, Houghton, D.D.S., Associate Dean, School of Dental and Oral Surgery, Columbia University, New York, N. Y.

Mead, Margaret, Ph.D., Assistant Curator, Ethnology, American Museum of Natural History, New York, N. Y.

Meiers, Joseph, M.D., Riga, Latvia.

Rioch, David McKenzie, M.D., Professor, Neurology, Head, Dept. of Neuropsychiatry, Washington University Medical School, St. Louis, Mo.

Rogoff, Julius M., Ph.G., M.D., Sc.D., Professor, Endocrinology, University of Pittsburgh, School of Medicine, Pittsburgh, Pa.

Schmidt, Leon H., Ph.D., Director, Research, Christ Hospital, Institute for Medical Research, Cincinnati, Ohio.

Sheehan, Donal, M.D., D.Sc., Professor, Anatomy; Director, Anatomical Laboratories, College of Medicine, New York University, New York, N. Y.

Simpson, G. G., Ph.D., Associate Curator, Vertebrate Paleontology, American Museum of Natural History, New York, N. Y.

Springer, N. Norton, Ph.D., Psychologist, Adolescents' Court, Brooklyn, N. Y.

Stekol, Jakob A., D.Sc., Assistant Professor, Biochemistry, Fordham University, New York, N. Y.

Travell, Janet, M.D., Instructor, Pharmacology, Cornell University Medical College, New York, N. Y.

Vance, Benjamin Morgan, M.D., Deputy Chief Medical Examiner, New York, N. Y.

Walton, Seth T., V.M.D., Ph.D., Director, Laboratories and Research, City Health Dept., Charlotte, N. C.

Ware, Ethan Earl, M.S., Professor, Zoology, Head, Dept. of Chemistry, Carnegie Institute of Technology, Pittsburgh, Pa.

Warner, John C., Ph.D., Professor, Chemistry; Head, Dept. of Chemistry, Carnegie Institute of Technology, Pittsburgh, Pa.

Watson, John B., Ph.D., Psychologist, William Esty and Company, New York, N. Y.

Weidensall, Jean, M.D., St. Simons Island, Georgia.

Weinrich, Morris F., Ph.D., Professor and Chairman, Dept. of Physics, Brooklyn College, Brooklyn, N. Y.

Wilkerson, Albert S., Ph.D., Instructor, Geology and Mineralogy, Rutgers University, New Brunswick, N. J.

Worne, Howard E., Ph.D., Director, Biological and Research Laboratories, Colton Chemical Corporation, Orange, N. J.

ASSOCIATE MEMBERS

Ecker, Enrique E., Ph.D., Associate Professor, Institute of Pathology, Western Reserve University, Cleveland, Ohio.

Eckstein, Gustav, D.D.S., M.D., Associate Professor, Physiology, University of Cincinnati, College of Medicine, Cincinnati, Ohio.

Edwards, A. S., Ph.D., Professor, Psychology, University of Georgia, Athens, Ga.

Eisenberg, Moses Joel, D.M.D., Grove Hall, Boston, Mass.

Heminway, Caroline E., A.M., Assistant Professor, Dept. of Geology, Smith College, Northampton, Mass.

Marshak, Robert E., Ph.D., Instructor, Physics, University of Rochester, Rochester, N. Y.

Sandiford, Irene, Ph.D., Professor, Dept. of Medicine, Billings Hospital, University of Chicago, Chicago, Ill.

Stewart, Colin C., Ph.D., Brown Professor, Physiology, Dartmouth College, Hanover, N. H.

Waldron, R. A., Ph.D., Professor, Botany, Penn. State Teachers College, Slippery Rock, Pa.

Warren, A. E., Ph.D., Assistant Professor, Biology, McMaster University, Hamilton, Ontario, Canada.

Weil, Leopold, Ph.D., Research Chemistry, Franklin Institute, Philadelphia, Pa.

Weinman, David, M.D., Instructor, Comparative Pathology and Tropical Medicine, Harvard University Medical School, Boston, Mass.

Weiss, Paul A., Ph.D., Associate Professor, Zoology, University of Chicago, Chicago, Ill.

Wetmore, Ralph H., Ph.D., Associate Professor, Botany, Harvard University, Cambridge, Mass.

Wheland, George W., Ph.D., Instructor, Chemistry, University of Chicago, Chicago, Ill.

STUDENT MEMBER

Schoenfeld, Nathan, M.A., Psychology, Graduate Student, Columbia University, New York, N. Y.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 JANUARY, 1940 No. 3

SECTION OF GEOLOGY AND MINERALOGY

DECEMBER 4, 1939

PROFESSOR PAUL MACCLINTOCK, Princeton University: *Weathering of Glacial Till.* (This lecture was illustrated by lantern slides.)

The weathering of a drift sheet is accomplished by the downward-percolating meteoric waters charged with oxygen, carbon dioxide, and humic acid. The results of this weathering, however, differ to a considerable extent depending on the climate, the parent material, and the topographic position. Warm, moist climate, with its accompanying vegetation, results in lateritic weathering; *i.e.*, leaching of silica and residual concentration of iron; whereas, cool, moist climate with its appropriate vegetation results in podsolic weathering; *i.e.*, removal of the iron to leave a highly siliceous deposit. The constitution of the parent material obviously plays an important rôle, which is to be evaluated in each area studied. The topographic situation, together with the porosity of the parent material determines the rate and depth of percolation. Free and deep percolation, as in upland situations in porous material, gives rise to well drained profiles, whereas lowland and/or tight material result in poorly drained profiles.

TRANSACTIONS of the New York Academy of Sciences, Series II, Volume 2, No. 3, January, 1940.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

The geologist has been able in some places to use the evidence from the depth and perfection of the profile to estimate the age of drift sheets. It, however, requires careful consideration of the variable factors mentioned above. Furthermore, in order to arrive at an age, it is essential that either the complete weathering profile be present or that the rate and amount of loss by surface erosion be known. If slope wash has removed the products of weathering as fast as they were formed, the record is obviously gone.

In New Jersey, there is Wisconsin drift, which has suffered so little weathering that only immature profiles are found, and an area of much older drift, which has suffered prolonged and deep weathering. However, in this latter area, the topography is so rolling that slope wash has removed material as weathering progressed and prevented the profiles from reaching maturity. As a consequence, judgment as to the age of the older drift is difficult. On the basis of its evidently prolonged weathering and the amount of erosion it has suffered, Salisbury originally considered most of it to be the equivalent of the Kansan, but a part of it to be intermediate in age between Kansan and Wisconsin. Later, he as well as Kummel thought the evidence for separation into two older drifts was not convincing and so placed it all in one glaciation. Later, Leverett revived the two-fold glaciation but extended Salisbury's intermediate drift considerably. The present study is an attempt at another technique of estimating age. Igneous pebbles, which are almost universally present in the drift were collected from uniform depth and uniform topographic position. They were then statistically analyzed as to the amount of weathering. The mean weathering of the pebbles in each exposure was then computed and plotted on a map. The resulting distribution seems to show three pre-Wisconsin drift sheets instead of two. This evidence from statistical analysis, however, must be considered in connection with other lines of evidence before final judgment can be passed.

SECTION OF BIOLOGY

December 11, 1939

Doctor Fred H. Albee, Orthopedic Surgeon, New York, N. Y., and Venice, Florida: *Comparative Biology of Bone Reconstruction.* (The speaker illustrated his lecture with slides and a striking motion picture in colors, depicting actual operations in bone-grafting, at close range. Ed.)

Tree grafting was already an ancient invention with the Babylonians when they descended upon the pastoral Israelites and carried them away into captivity. Had it not been for this incident, the art might have been buried in the dust of antiquity with the Babylonion Empire.

But, along with the art of commerce—of which the Babylonians were past masters—the Israelites rescued it, and centuries later passed it on to the Greeks and Romans. The latter, being experimentalists and perfectionists, developed the art and precision of grafting to such a degree that their ancient writers left records of having successfully grafted grape vines and apple twigs on to poplars, elms, and locust trees. Repeated experiments in modern times, however, prove that the chance of successful propagation of such ill-assorted unions is one in a thousand.

Grafting, today, is confined within certain limits. As a general rule, the scion from one tree will not succeed on every other tree because the essential natural affinity is lacking. There must be a close relationship and similarity of structure between the stock and the scion, either as varieties of the same species, such as different sorts of apples; or species of the same genus, as apple and pear; or genera of the same family, as cherry on the plum or peach on the almond tree.

In spite of the claims that species of the same genus—such as pear scion on apple stock—will not thrive, Mr. James Halloway of Long Island, N. Y., has succeeded, because of his precision technique, in grafting a scion of English stewing pear on

the dwarf stock of a French crab apple tree. Moreover, it has borne fruit for many seasons. Another apple tree in his orchard bears 38 grafted varieties of apples.

Grafting is based on the power of union between young tissues. When the parts, that is, the stock and the scion, are placed nicely in contact, the ascending sap of the stock passes into and sustains life in the scion. The buds of the scion become excited by this fresh supply of sap and begin to elaborate and send down woody matter, which, passing through newly granulated substance of the parts in contact, unites the scion firmly with the stock.

A similar process takes place in bone union. Only, in place of the sap, blood rushes from the host to the cells of the graft and union takes place, first by deposition of soft callus and then by impregnation with lime salts.

The biological principles of plant grafting remain inviolable in bone grafting, precision of technique being the keynote. But the more highly specialized the animal tissue is, the more imperative it becomes that the graft be autogenous.

Reestablishment of sap circulation in the plant is less complex than reestablishment of blood circulation in human bones. For of all the layers in a tree—alburnum, heart-wood, and heart—the alburnum is the most important, for it is directly responsible for sap ascension.

But in the bone, especially long bones, all the layers—periosteum, cortex, endosteum and marrow—take part in the union.

Seventy-five per cent of the blood-supply to the cross-section of the long bone is furnished via the marrow cavity and 25 per cent via the periosteum of that bone. For this reason it is desirable to contact the bone graft with the marrow on both sides of the bone cortex, if possible, and still more so with the various layers.

Because of the exactitude of technique required and the brittleness combined with the knurly character of the bone, the chisel and mallet first employed were found inadequate. An electrically-driven, automatic cutting apparatus was needed, for the very life of this type of surgery depended upon it.

I was the first to design such an electrically-driven shaping mechanism, now known as the Albee Bone Mill. This Bone Mill consists of power-driven automatic cutting instruments of the male and female type, insuring precision fit of the graft into the graft-bed with the faultless accuracy of a glass stopper in a bottle. This is most important, for compression from poor fit may kill bone cells.

This precision fit supplies mechanical immobilization, immediate reestablishment of blood supply from host to graft, and places the least burden upon the osteogenetic or callus-forming properties of the bone.

Comprising this Bone Mill are the following instruments: a *lathe,* which shapes living bone pegs of various sizes; *dies,* which cut threads upon these pegs, converting them into screws; *taps,* which cut threads upon the drill holes; and a *universally adjustable twin saw,* which cuts graft beds for inlays of any size or shape.

This mechanism enables the surgeon to shape bone at the operating table under sterile conditions with the speed, facility, and accuracy of a machinist.

Relying primarily on precision technique, successful results have been obtained in cases where fourteen previous operations had failed.

Tibial bone having the highest degree of callus-forming property is usually employed for bone grafts. When it is insufficiently large, however, graft is taken from the ilium.

The only displacing force to be counteracted in the plant is that of the wind, while, in the bone, there is a constant pull of muscles, as well as their exaggeration by reflex from pain.

Sequential to union of tissues, either in plant or bone, there follows a series of mysterious processes summarized under Wolff's Law of Stress.

Under this law, the graft remakes its internal structure, alters its shape, and takes on volume and strength, in order to accommodate itself to the conditions and stress demanded of it by its new environment.

For instance, a graft the size of a lead pencil, when implanted to replace a portion of the thigh bone, because of stress demands, will take on the characteristics of the thigh bone shaft in strength, size, shape of cross-section, external contour, and internal architecture.

More than any other field, bone grafting emphasizes the importance of strict adherence to biological principles.

Credit for Source Material

Scientific American, Fred H. Albee, "Bone Surgery With Machine Tools." April, 1936.

Mr. James Halloway, 48 Dosoris Lane, Glen Cove, Long Island, N. Y.

Country Life Nurseries, Garden City, Long Island, N. Y.

REPORT OF THE ANNUAL MEETING

DECEMBER 13, 1939

The 121st Annual Meeting of the Academy for the election of Officers and Fellows, the presentation of reports and the transaction of other business was held at The Hotel Astor on the evening of Wednesday, December 13.

The Corresponding Secretary reported that there are now upon the rolls of the Academy 47 Honorary Members and 6 Corresponding Members. Two deaths were reported during the past year.

The Recording Secretary reported that, during the year 1939, the Academy held 8 Business Meetings and 26 regular Sectional Meetings, at which 27 stated papers, all of very high quality, were presented. Besides the Annual Dinner, the Academy acted as host at a presentation in the Hayden Planetarium arranged for the delegates attending the fiftieth anniversary celebration of the founding of the Geological Society of America. A lecture and informal reception was held on behalf of the Annual Meeting of the American Anthropological Society last December. Four informal receptions were held under the auspices of the various Sections of the Academy in honor of prominent out-of-town speakers.

The Section of Physics and Chemistry completed a most successful year made noteworthy by three two-day conferences on special subjects of research. They were attended by the most representative research workers in the respective fields from various parts of the United States.

Besides the above, an important two-day Astronomical Conference was held under the leadership of Professor Harlow Shapley, Director of the Harvard Observatory; Professor Henry Norris Russell, of Princeton University; and Professor Jan Schilt, of Columbia University, on the subject of "The Internal Constitution of the Stars." This marked an important extension of the conference idea in the Academy and won

the enthusiastic support of the twenty-six representative astronomers in attendance. This conference was the direct result of the important series of inquiries on the source of stellar energy initiated by President Morrison in connection with the astronomical prize competitions established by him during the past fifteen years.

The Recording Secretary thanked the Members of the Academy for their continued and increasingly active support of the undertakings of the past year. This is evidenced, not only by their steady demand for the publications of the Academy, but also by the impressive fact that more than 2,200 Members attended the meetings during the above period.

One Life Member, 7 Sustaining Members, 269 Active Members, 10 Student Members and 213 Associate Members were added to the rolls. Thus a total of 500 new members was added during the year.

The membership of the Academy is at present 1190, which includes 1 Patron, 96 Life Members, 162 Annual Sustaining Members, 470 Annual Active Members, 60 Student Members, 348 Associate Members, 47 Honorary Members and 6 Corresponding Members. Of these 202 are Fellows.

The Editor reported that one complete volume of the *Annals* and three parts of another have appeared. They are as follows:

"Molluscan Faunas of the Domengine and Arroyo Hondo Formations," by Harold E. Vokes, pp. 1–246. This paper constitutes the entire Volume 38.

The following three parts of Volume 39 were published:

"The Organization of The New York Academy of Sciences," pp. 1–28.

"The Morphology and Functional Evolution of the Atlas-Axis Complex from Fish to Mammals," by Francis Gaynor Evans, pp. 29–104.

"Electrophoresis," by Harold A. Abramson, Edwin J. Cohn, Bernard D. Davis, Frank L. Horsfall, Lewis G. Longsworth, D. A. MacInnes, Hans Mueller, and Kurt G. Stern, pp. 105–212.

The fourth part of Volume 39 entitled, "American Cities and States: Variation and Correlation in Institutions, Activities, and the Personal Qualities of the Residents," by Edward L. Thorndike, pp. 213–298, is scheduled to appear on December 22, 1939.

This year, the Academy established a new series, bound in cloth, to be known as *Special Publications.* The first volume of this series entitled "Climate and Evolution," by William Diller Matthew, pp. 1–224, has been published.

Three parts of "The Scientific Survey of Porto Rico and the Virgin Islands" have been printed:

Volume VIII, Part 3, "Diatomaceæ of Porto Rico and the Virgin Islands," by Robert Hagelstein, pp. 313–450.

Volume XIV, Part 3, "Insects of Porto Rico and the Virgin Islands," by Harry G. Barber, pp. 263–442.

Volume XVI, Part 2, "Polychaetous Annelids of Porto Rico and Vicinity," by Aaron L. Treadwell, pp. 151–320.

Volume XVIII, Part 1, "Porto Rican Archaeology," by Froelich G. Rainey, is now in galley proof, and will be ready for distribution early in January, 1940.

The last seven numbers of Volume 1, Series II, of the TRANSACTIONS, together with the title page and index for the 136-page volume have been printed and distributed.

The total number of pages printed to date since the first of the year in the *Annals, Special Publications,* "Scientific Survey of Porto Rico and the Virgin Islands," and TRANSACTIONS, combined is 1,302.

The Librarian reported that 7,928 separate publications were distributed to the members of the Academy, authors, exchanges and the sales list, classified as follows: *Annals,* 2,989; "Scientific Survey of Porto Rico and the Virgin Islands," 814; *Special Publications,* Volume I, "Climate and Evolution," 540; TRANSACTIONS, Volume I, 2,357; and "Man in a Chemical World," 1,228.

The Library of the Academy has received 2,407 publications from its exchange institutions.

The Treasurer reported that the surplus for the fiscal year ending November 30, 1938, was $14,762.00. During the past year ending November 30, 1939, receipts from all sources amounted to $15,718.14, making a total with the amount transferred above of $30,480.14. The disbursements for the past year amounted to $21,306.39, leaving a surplus as of November 30, 1939, amounting to $9,173.75.

The A. Cressy Morrison Prizes of $200.00 each for the two most acceptable technical papers in a field of science covered by the Academy or an Affiliated Society were awarded to the following papers entitled:

"Studies on the Nutrition of Colorless Euglenoid Flagellates," by Henry W. Schoenborn, New York University.

"The Structure and Relationships of *Protoceratops*," by Barnum Brown, The American Museum of Natural History, and Erich M. Schlaikjer, Brooklyn College.

Honorable Mention was awarded to the paper entitled "The Problem of the Scattering of the Fast Electrons," by John A. Wheeler, Princeton University.

Announcement was made that two prizes of $200.00 each will be awarded under similar terms at the Annual Meeting of December, 1940, and that an additional prize of $500.00 will be awarded at the same time to the most acceptable paper on the general subject of the sources of stellar energy. The precise terms of the competition will be published in a later issue of the TRANSACTIONS.

The following members were elected to Fellowship:

Fred H. Albee, M.D., Sc.D., LL.D.
Harold L. Alling, Ph.D.
Percy Laurance Bailey, Ph.D.
Russell L. Biddle, Ph.D.
McKeen Cattell, Ph.D., M.D.
Charles G. Darlington, M.D.
Gustav Egloff, Ph.D.
Charles E. Hadley, Ph.D.
John Eric Hill, Ph.D.
Frances Holden, Ph.D.
Laurence LaForge, Ph.D.
Victor K. LaMer, Ph.D.
Ralph Linton, Ph.D.
Harold N. Moldenke, Ph.D.
Frederick Osborn, A.B.
Thomas M. Rivers, M.D.
William J. Robbins, Sc.D., Ph.D.
Joshua Rosett, M.D.
Fordyce B. St. John, M.D., Sc.D.
Hugo H. Schaefer, Phar.D., Ph.D.
Rudolph Schoenheimer, M.D.
Theodore Shedlovsky, Ph.D.

Charles P. Smyth, Ph.D.
Daniel Starch, Ph.D.
Mary B. Stark, Sc.D., Ph.D.
Byron Stookey, M.D.
Harden F. Taylor, Sc.D.
George C. Vaillant, Ph.D.
Orlando F. Weber

Honorary Membership was conferred upon the eminent astronomer, Henry Norris Russell, Ph.D., ScD.

The following officers were elected:

President
Roy Waldo Miner

Vice-Presidents
Leslie E. Spock
Anne Anastasi
Horace E. Wood, 2nd
Ralph Linton
Victor K. LaMer

Recording Secretary
Duncan A. MacInnes

Corresponding Secretary
C. Stuart Gager

Treasurer
Wyllys Rosseter Betts, Jr.

Librarian
John Hendley Barnhart

Editor
Erich M. Schlaikjer

Councilors (1940–1942)
W. J. V. Osterhaut
Douglas W. Johnson

Finance Committee
Herbert F. Schwarz
John D. Sherman, Jr.
Wayne M. Faunce

After the Business Meeting, the following program was presented:

Address by the Retiring President
"The Practical Benefactions of Pure Science"
by
President A. Cressy Morrison

"Flight to the Stone Age"
by
Richard Archbold

Abstracts of these papers, and also those of the A. Cressey Morrison prize winners, are included in this issue of the TRANSACTIONS.

NEW YORK ACADEMY OF SCIENCES

Address of the Retiring President* given at Annual Dinner and Meeting

December 13, 1939

President A. Cressy Morrison, New York, N. Y.: *The Practical Benefactions of Pure Science.*

The purpose of this address is to demonstrate the magnificent usefulness of knowledge gathered for its own sake, by showing some of its applications.

Brevity is necessary. I will therefore present but a few illustrations and discuss them as tersely as possible. Each illustration of necessity opens up vistas which are endless.

Thought is the most important force in the world. Thought gives rise to ideas. Ideas are more permanent than their materialization in visible things. A structure may easily be destroyed, but the idea which gave it birth may become immortal. Civilization, in all its ramifications, is a direct outgrowth of the vast accumulation of ideas. Pure science and its application is a manifestation of that something back of the human mind, which I believe demonstrates the existence of a supreme intelligence.

It so happens that a century ago, Goodyear and Daguerre made their very important discoveries. The connection between Goodyear's vulcanization of rubber and the airplane may be obscure to some, but it is direct. Latex to rubber, rubber to the pneumatic tire, the pneumatic tire to the perfected automobile and its light but powerful internal combustion engine, without which the airplane today would not be able to fly. Goodyear, therefore, has put the world on wheels, enormously developed our roads, and lifted Man into the air.

Daguerre discovered photography—step by step it has ad-

* Mr. Morrison served as President of the Academy during the years 1938–1939.

vanced. As early as 1869 the process which awaited the development of the film and the proper chemicals to produce picture in color was announced. The moving picture industry rests upon his discovery and its effect upon the world is already apparent. Who would have supposed that Daguerre's discovery, as now developed, would so improve the accuracy and facilities of astronomy that nebulae have now become island universes, space has grown from thousands to millions of light years, new radiations have been recorded, and photography has become the handmaiden of astronomy.

Michael Faraday's discovery of the principle of electromagnetic induction, was for him, the reward of an excursion into the unknown; the fruit of pure reason; an idea embodied ultimately in an experiment. To Faraday's contemporaries his discovery meant little or nothing. To those of us who live in this electrical age, Faraday's toy has become the foundation of a new civilization. It showed the way to harness the vastly powerful forces of electricity. Light, heat, power, and all the wonders which flow from the millions of electric dynamos and motors, which lift burdens of toil from our shoulders, delight and amuse us, employ simply and directly the principles of Faraday's discovery.

Whatever may have been Faraday's gift of prophecy, it is certain that he was led to this discovery, as to others, by an insatiable desire to learn and to know. No hope of other reward was needed to goad his active imagination. No vision of immediate usefulness spurred him in the search for truth.

Quite obviously, in our fast moving age, the long gap between Faraday's discovery and its application to human needs has been materially shortened by the development of vastly intricate industrial machinery to feed upon and utilize the results of researches in pure science. Obviously, too, the wealth created by industry is supplying the essential support and stimulus to research in pure science. President Conant of Harvard has described this relationship by a happy analogy drawn from the field of biology. He terms the mutual interde-

pendence of pure science, pursued for the purpose of increasing human knowledge and industry, whose objective is the creation of wealth in the broadest sense of that word, as "symbiosis" (living together). To make this meaning clear, he illustrates symbiosis by describing the process by which lichens live. A tender green plant synthesizes food for itself and also for a strong colorless fungus whose hardiness protects both from destructive forces. The application of the figure to the present subject and its aptness in describing it are evident.

Each phase of knowledge is connected by strings, often invisible, to a previous state of knowledge. Hence, we must give considerable credit to astronomy for things that are now of everyday use. Let me give some instances of fundamental ideas essential to further development of science, which have come from astronomy. The connections are so remote as to have become practically lost.

A mathematical equation devised by Clerk Maxwell has given us radio and television. The velocity of light was first discovered by Roemer from observations of the eclipses of Jupiter's satellites, and the notion of velocity of light or radiation was essential to the formulation of Maxwell's laws. A second instance of a very remote connection is Newton's law of gravitation and the principles of mechanics, which were arrived at on the basis of the motion of the moon. It is these principles that are the base of the entire complex of mechanical things that we possess at the present time.

Astronomy made necessary the study of optics. It has developed the strength of the human eye until it has become a two hundred inch reflector. It has, in the other direction, aided in the development of the microscope, until all the beneficent results of microscopic investigation are now in our possession and the end is not yet. The development of the study of optics and the correction of the imperfections of the human eye have given us the universal use of ordinary eye-glasses which has become one of the great factors in safety, in human enjoyment, and the advancement of education. Let us not forget, how-

ever, that the Chinese were two thousand years ahead of western civilization in the use of eye-glasses.

Navigation, aviation and surveying are based on the astronomy of the ancients, as was the discovery of the fact that the earth is a sphere. The calendar and the accuracy of our time are daily contributions of astronomy. The development of mathematics by the necessities of astronomy and the contributions of astronomy and physics to mathematics constitute a mutual obligation of great use to the world.

It is impossible to predict future uses of today's astronomy and doubtless many phases of it will never have any direct bearing on our daily life. However, modern spectroscopic investigations of matter in tenuous nebulae enable us to learn the behavior of matter under conditions that cannot yet be realized in any laboratory. In this field there is at least a probability of tangible results already forecast by the great practicability of many applications of extremely high vacuum.

We are indebted to astronomy for the spectroscope, which discloses with marvellous accuracy the constituents of matter found throughout the universe, but few realize that this same instrument is now in practical use in hundreds of laboratories, where it is disclosing immeasurably small amounts of impurities which may be useful or harmful in substances heretofore considered as pure. The majority of astronomers, if asked "What is the use of astronomy?," would probably quote Poincare, "L'astronomie est utile parce qu'elle est belle" (astronomy is useful because it is beautiful).

If we study the work of Copernicus and the impact of Galileo's pronouncement that the earth is not the center of the universe upon the ideology of the world, we find that it has changed and is changing the philosophy of every human being. If correct thinking is an advantage, perhaps in the field of ideology, one of the most valuable contributions of astronomy is the reaction to the emphasis which Galileo's discovery placed upon the insignificance of Man. This, at first resented, has now become an inspiration because of the better understanding

of the human intellect, its marvels and its complete superiority to the material. To force a realization of the amazing stimulus to research and the overwhelming effect of ideology upon Man, one needs but mention Darwin.

I cannot refrain from relating a curious fact regarding certain races of an age still veiled in the mists of antiquity. The wheel is so much of a commonplace that we seldom become conscious of its fundamental usefulness or the startling nature of its invention. The wheel was invented before history began. Its usefulness to Man and civilization is an unparalleled gift. To say that the wheel is the fruit of pure science would startle most of us, and yet the genius and research required for its development undoubtedly compare with some of the great discoveries of modern time.

To emphasize this point, it is only necessary to call attention to the fact that the civilization of the Mayas, Aztecs, and Incas was independently comparable with the best of the rest of the world at the time. It should be added that in mathematics and astronomy they had developed the fundamentals to so high a degree that their measure of time and the accuracy of their observations were equal to the best which civilization had developed elsewhere. These civilized groups in the Americas, dating back twenty thousand years, had enough genius to simplify their mathematical problems by their independent discovery of the cypher or its equivalent, and yet no wheel turned in America until the idea was brought to the new world by Columbus and his successors. One marvels that a civilization in prehistoric America possessed of such abilities in this field, as well as in other applications of science, should yet have been so backward mechanically as not to have invented that immensely useful device, the wheel. Our indebtedness to the obscure genius who invented and made practical the wheel is so great that we can never pay our obligations, but so far no monument has been erected to this pioneer in pure science.

Perhaps no single scientific discovery has had today such enormous and so valuable use as catalysis; invaluable tool of

chemistry. Indeed, there are few processes of chemical industry which do not employ such a promoter of one kind of another to accelerate and direct chemical reactions toward desired ends. Even the simple, universal reaction of combustion requires the presence of a minute amount of water vapor to allow it to proceed.

The ancients visioned the invaluable usefulness of the promoters of chemical reactions we now designate as catalysts and sought a universally active substance of the kind under the name of the "philosopher's stone." The advent of chemistry as a science directed attention toward these traces of extraneous substances necessary to bring about certain chemical reactions, and, more than a century ago, Berzelius had accumulated enough information on the subject to give phenomena of this type the name of "catalysis." Berzelius and a host of other investigators in the decades that followed pursued these inquiries without expectation of reward, but no intellectual pursuit has had more significant consequences. Sulfuric acid, as universally valuable in chemical manufacture as pig iron is in mechanical industry, is produced in our own country by the millions of tons annually by processes utilizing platinum, vanadium oxide and other materials as catalysts. Enormous tonnages of cottonseed no longer are wasted to clog streams and foul landscapes, because a catalytic process easily converts their oil content into a palatable, nourishing solid fat. The first World War was begun only after Germany was assured independence of imported nitrate for manufacture by the perfection of Haber's catalytic process for making the air supply this military essential. Though a military essential, how much more important is the fact of an unlimited supply of nitrogen to fertilize our cultivated fields and ultimately save the world from starvation. Our now commonplace, but still amazing, conquest by air of immense distances over continents and seas depends upon catalytically prepared fuels and catalytically controlled burning of them in internal combustion engines. What is true of air-borne commerce is quite as true of highway traffic in vehicles powered by similar fuels.

If the useful applications of catalysis stopped there, its value would already be incalculable. But it is only fair to say that these are but samples. They were chosen because they are clearcut. Beyond them stretch vast fields of catalytic reactions which lie at the very base of life itself. Because the processes involved in living organisms are intricate and complex beyond our present understanding, it is scarcely justifiable to class them with such simple chemical reactions as the synthesis of ammonia, the manufacture of sulfuric acid and the hardening of fats.

Yet the significant effects of minute traces of foreign matter on life processes point to their close kinship with other catalyses. The failure of certain farms to produce premium melons now, as of old, was ultimately traced to a depletion of the tiny traces of manganese in the soil. Traces of this element, so small that ordinary analysis failed to reveal them, were found essential to the growth of full-flavored melons. More intricate and equally essential are the vitamins, the hormones and the auxins which, in minute traces, control life processes. These are rapidly becoming tools of immense value in the hands of physicians and agricultural scientists as well, to improve all living creatures. Man, the animals, and Nature's bounteous luxury of plants are being made healthier, stronger and more useful by the applications of these products of pure chemistry.

A classical case in organic chemistry serves to illustrate the development of a chance discovery by an intelligent intellect actively inquiring into a fundamental fact of incalculable theoretical and practical importance. This is "substitution" or the replacement in an organic compound of an atom (usually hydrogen) by an atom of another element or by a radical (a group of atoms of two or more elements). Substitution was discovered by Dumas, when he was asked to find out why the candles at a ball in Paris gave off suffocating fumes when they were burning. Dumas learned that the wax had been bleached with chlorine, and he found that chlorine had evidently re-

placed some of the hydrogen of the wax, yet had not destroyed its essential nature. His published paper on what he had found is one of the classics of chemistry, but the idea in it was so new that it was met with jeers. It inspired the publication, in Liebig's Annalen, of an ironical paper written by Wöhler, but signed "S. C. H. Windler" (Swindler) who described some wholly imaginary experiments with a piece of cotton cloth. Step by step he replaced the hydrogen, oxygen and carbon of the cloth without changing its appearance or other properties, so that in the end the piece of cloth was composed of pure chlorine, yet indistinguishable from the original material.

In spite of the early disbelief and ridicule, the fact of substitution was soon well established and its application has influenced the greater part of the development of organic chemistry in the factory as well as in the laboratory. Without substitution, the chemist could never have produced the synthetic dyes or the hundreds of invaluable medicines. It is commonly said that the dyes are obtained from coal tar. The truth of this misleading statement is that a few essential compounds obtained from coal tar are the raw materials from which are built up, step by step, with the aid of substitution, the complicated molecules which meet hundreds of human needs. The enormous development of synthetic organic chemistry is the outgrowth of a simple but fundamental discovery.

The photoelectric effect was discovered by Hertz and Hall in 1887 and 1888 respectively, more or less through difficulties with experiments carried out for other purposes. The photoelectric effect underlies the sound effects accompanying moving pictures and it is absolutely essential for television.

"Marconi was inevitable," according to Abraham Flexner. "The credit for what has been done in the field of wireless belongs, as far as such fundamental credit can be definitely assigned to anyone, to Professor Clerk Maxwell, who in 1865 carried out certain abstruse and remote calculations in the field of magnetism and electricity. Maxwell reproduced his abstract equations in a treatise published in 1873. Other dis-

coveries supplemented Maxwell's theoretical work during the next fifteen years. Finally in 1887 and 1888 the scientific problem still remaining—the detection and demonstration of the electromagnetic waves which are the carriers of wireless signals—was solved by Heinrich Hertz, a worker in Helmholtz's laboratory in Berlin. Neither Maxwell nor Hertz had any concern about the utility of their work. They had no practical objective. The inventor in the legal sense was, of course, Marconi. But what did Marconi invent? Merely the last technical detail, the now obsolete receiving device called a 'coherer,' almost universally discarded.'' Yet no man will deny him the vast credit due for making pure science in this field of supreme importance to the world.

Edison's experiments with a metal plate carried on a platinum wire and placed alongside the filament in one of his electric lamps, led to the discovery of the so-called ''Edison effect.'' His discovery, that when the plate is connected through a galvanometer with the negative terminal of the filament, no current will flow, but if it is connected with the positive end of the filament, a current will flow steadily through the galvanometer to the plate as long as the filament remains heated, is the basis of present-day vacuum tubes in universal use for radio communication and other purposes.

Entomology, devoted to the study of insect life, has already greatly aided humanity to win the bitter struggle against the vast hordes of insects competing for our food supply. A most striking illustration of the effective work of entomologists is the recovery of the continent of Australia from threatened depopulation by the swift spread of a fast-growing cactus. A million acres of valuable land were being occupied annually by cacti in an advancing wave no ordinary measure would stop. The entire continent was in danger of being engulfed, as sixty million acres had been, when entomologists suggested employing an insect which lives only on cactus plants. The *Cactoblastis cactorum* was finally brought in from Central America and by virtue of its prodigious appetite and enormous fecundity Australia has been saved for Man.

Australia was separated for millions of years from the mainland, and developed a balanced individual flora and fauna, but these had no protection when new plants, new animals or new insects were brought into Australia. So simple an animal as the rabbit became a pest. We may smile at the entomologists who measure the probosces of butterflies and bees, but the practical application of their studies becomes immediately apparent, as is well-known in connection with that very useful plant, red clover. It seems that Australia set out to grow clover, imported the seeds, and had successful crops for a year or two, and then the crops failed. It was then that the entomologists came to the rescue and imported bumble bees, the only insect that can fertilize red clover, and now clover grows.

Apropos of clover, one thinks of alfalfa. Pure research discovered that in Siberia, far north of the line where alfalfa can be grown in the United States, a species of alfalfa would survive. As a result of its importation and development, a strip 100 miles wide and 3,000 miles long in the United States can grow alfalfa successfully where none would grow before.

Novius cardinalis is a lady beetle imported from Australia to aid in the control of the scale insect *Icerya purchasi,* a pest of citrus and other fruit trees. It proved so efficient in California that it has now been imported to control the scale in all countries where scale is a pest.

The study of plant breeding, plant diseases and fungi has doubled the yields, improved the quality and often prevented the total loss of many of our most important crops. Can more be said for pure science and its applications?

The life history of the cattle tick discovered by Theobald Smith disclosed a method of control which is of untold value in preserving our food supply.

The theory of Sir Donald Ross and the story of how yellow fever was conquered as a result of the heroism and self-sacrifice of soldiers in our Army is too well known to be repeated here, but this work of the Army saves a million lives each year, or more than are killed by war.

Geology and paleontology are so closely linked that it is difficult in so brief a paper to adequately separate them.

The utility of geology as a means to the discovery of metals, minerals, oils, and the location and physical characteristics of materials for dams and other structures is so apparent that we cannot walk a paved street anywhere in the world but that its geological relationship echoes with every footstep. The search for the remains of prehistoric animals had led to discoveries of prime importance in all directions. But civilization, while based on material things, is equally dependent upon the increase of human knowledge. The greater our understanding of the structure of the earth and the history of living creatures through an almost infinite past, with their ultimate relationship to Man, the more the mind of every thinking person broadens, better judgment develops into tolerance and understanding. These contributions may do more for the elevation of the mind of Man, the real objective of human life, than can be gained from developments which add merely to Man's security and comfort. Let us then tread the earth with reverence.

Early attempts to measure the constant of gravitation using the torsion balance provided experience which has made possible one and perhaps the most important form of geophysical prospecting now used to find oil.

Dalton's Atomic Theory and Mendeleeff's Periodic Law developed the idea that all matter was composed of a limited number of kinds of indivisible building bricks, known to us as the atoms of the elements, out of which all things are constructed. While these concepts were indispensable as stepping stones to a better understanding of the structure of matter, they seemed to close the door to the possibility of anything outside of the material.

The discovery of radium smashed these bricks and transformed them into little constellations with a central nucleus, about which revolved electrons at infinitesimal, but relatively great distances from the nucleus. This opened the door to a

better understanding of forces, probably enormously more important to the ultimate destiny of Man than the material benefits which have already so magnificently followed the discovery of radio-activity in all its present applications. The unbelievable results derived directly from the discovery of X-rays are too apparent for emphasis.

One need but mention the ocean and the seven seas to bring to mind the unlimited fields for further research in the ocean as a source of food supply. How little we appreciate that the ocean and deposits of ancient seas are sources of our great preservative, salt, and how many think further as to the indispensable use of salt as a source of chlorine and caustic soda, both essential for the continuance of our chemical industries. How grateful we should be for chlorine which makes our vast city water supply safe and has almost conquered typhoid fever. Few know that the study of the locomotion of fishes has added to the speed of airplanes.

Investigation of the electrochemical equivalences of metals and alloys yielded data upon which are based the principle of using a thin film of one metal to protect another metal or alloy against corrosion. The most familiar example is the use of zinc or tin on iron. The corrugated iron roof of the tropics and the ubiquitous tin can are the result. No one can measure the effect on our food supply of the tin can.

I can draw from personal experience some illustrations of astonishing developments due to the electric furnace, beginning only as far back as the eighteen-nineties.

Gibbs' Phase Rule, which grew out of his profound studies of chemical and physical equilibrium, is the guiding principle in much of our development of alloys and it has certainly had a great influence over modern metallurgy. The real greatness of Josiah Willard Gibbs is slowly but surely becoming realized and, as an American scientist, he now takes his place as one of the first mathematical physicists of all time.

In considering the blast furnace as it was and is, we realize that its high temperature has, for certain reactions, been super-

seded by the higher temperatures of the electric furnace. Temperatures approaching 7000° Fahrenheit have been utilized for the production of calcium carbide and the ferroalloys. These alloys are intimate mixtures of certain elements with iron, and their usefulness is extraordinary. Calcium carbide, hence acetylene, has developed a universally used implement of industry, known as the welding and cutting torch. Acetylene is an endothermic compound, and when burned with oxygen gives the highest temperature in chemistry (6000°). This tremendous heat literally melts metals so that they fuse—hence welding. The details of the uses of this marvellous tool, a complete innovation, would make a volume in itself. Calcium carbide in combination with nitrogen offered the first practical solution of the nitrogenous refertilization of soils exhausted by over-cultivation. While, in this field, calcium cyanamide has been superseded to a large degree by other processes, nevertheless, it was the first practical answer to the vital question of the continuance of human life on the planet. Acetylene has become the base for large chemical industries and its usefulness is developing continually.

The very important alloys, ferrosilicon and ferrochromium, were first produced in the electric furnace in America, followed by ferrotungsten, ferromolybdenum, ferromanganese and many others, as well as by the invaluable artificial abrasives. The amazing effect of small quantities of alloying elements on steel would test credulity were it not daily demonstrated. The alloyed high speed tool used in working metals has accelerated many machining operations. A tool alloy of tungsten, chromium and cobalt has greatly hastened the cutting of metals, and a related alloy has more than doubled the life of wearing parts. The advent of aluminum, which is an electric furnace product, and its alloys has answered a thousand economic questions. An illustration not widely known but showing the usefulness of alloys is provided by the tools of cemented tungsten carbide that are used in the drilling of oil wells. This alloy has so increased the economy by which wells can be bored

that many thousands of feet have been added to the depth readily attainable. Thus enormously valuable new strata of the earth have been added to a field of exploration that, of course, comprises the whole land area of the earth. What this means is beyond our imagination. Vast deposits of the now universally essential crude oil and gas, and untold reserves of metals and materials are sure to be disclosed. Thus the whole development of metallurgy and economic advances are profoundly influenced by the alloys.

The Joule-Thompson effect deals with the change of temperature when a gas expands from a high pressure to a lower one. It is the basis of the process of liquefaction of gases and our present air-conditioning and refrigeration industries. I doubt whether any reader of the original scientific papers describing this discovery could have had any idea of the tremendous potentialities of the simple "porous plug" experiments as originally carried out.

Priestley's discovery of oxygen was followed by controversy, but he nevertheless was proved to be correct. Oxygen is our most abundant and active reagent. It has kept the hearth fires of the world burning since Man's beginning, yet its prodigious usefulness as an element had to await the discovery of a process separating it from the nitrogen and other gases of the air. Here again, research based upon the original experiments brought the answer in the liquefaction of air. Within the memory of the present generation, the discovery of the great industrial usefulness of oxygen as an element has been followed by a growth almost unparalleled in industrial history. It is essential in the oxyacetylene welding torch and it is equally essential in the cutting and shaping of steel parts. It has done its necessary part in replacing the tedious and almost impossible saw with a chemical knife that cuts steel almost as readily as an ordinary knife cuts cheese. Several billion cubic feet of oxygen are used each year. A hundred million cubic feet are used yearly in our hospitals, saving life and alleviating pain. Carrying this work further has brought

to light the usefulness of argon and neon, since electrified neon is the source of lights for the illuminated signs which now greet the eye everywhere. Argon is less conspicuous in one sense, but far more useful. The many years of pure research on the tungsten filament, glass and all the gases, which proved that argon could be used in the ordinary electric light bulb now so universal, has borne wonderful fruit.

In 1917, helium was a chemical curiosity that had been produced only in small quantities by investigators in pure science. Its discovery on earth had followed the findings of helium in the sun. On our earth where it could be bought at all, it then cost about $2,000.00 per cubic foot. However, technical men saw the possibility of extracting helium from natural gas for use in lighter-than-air craft and suggested it for military aeronautics. An efficient extraction process using the same principles was developed, by which helium could be secured in quantity at low cost. Helium is now produced at approximately one cent per cubic foot and in quantities sufficient for many uses.

In addition to the use of helium as a non-combustible lifting gas for airships, it is valuable for deep-sea diving and for medical purposes. Mixtures of helium and oxygen prevent caisson disease. Within the past year, new diving records have been made using helium. The medical profession now prescribes helium mixed with 20 per cent oxygen as a breathing atmosphere for persons afflicted with asthma, and this gives almost immediate relief. This humanitarian use of a once "rare" gas offers wide possibilities of medical application, according to reports by doctors who are studying the effects of helium on respiratory diseases.

The very fine work of separating helium from the mixed gases known as "natural gas" as it comes from the earth, meant the catching of one flying atom in every two hundred and passing on one hundred and ninety-nine to be used as fuel. This research taught us how to separate in purity all the other gases of the mixture, and on this rests our greatest organic

industry and one of ultimate supreme importance to the human race. By the use of natural gas and of waste gases from petroleum, by the use of coal and lime, and ultimately of oil shale, we are already producing in great variety extremely valuable chemicals and plastics, as well as textiles like silk or wool, and from these gases we shall ultimately produce food and drink.

This address is chiefly notable for what it of necessity omits, but these examples drawn from many fields of purely scientific inquiry show in unmistakable terms the enormous value of giving the searching imagination free rein. Even the calculations of astronomers accustomed to huge figures could not evaluate the wealth of comfort, convenience, health and happiness that stem from the researches I have mentioned. To forecast their effects on future generations is beyond human powers, great as these have been shown to be in other directions.

One is led by such speculations to the immediate conclusion that in this way lies peace on earth. Peace lasting through eternity because it will be based, not on petty jealousies and greed, but soundly on the sure foundation of plenty. The ancient prophet's vision of swords beaten into plowshares and spears into pruning hooks was the product of an agricultural age. Today, we envision secure and permanent peace based on turning a part of the wealth of nations, not into armaments, but into the manifold tools of science.

The irresistible conclusion to be drawn from the study of pure science and its application is that human thought, crystallized into ideas and materialized for the benefit of Man, is the foundation of every advance in civilization.

Science must begin with the student, and every opportunity must be given youth to gain a foundation for a life of Science. If human advance is to continue, the student must be liberally supported. He should be encouraged to visit museums where his interest will be aroused by the work of others. Our educational institutions have shown conclusively that they deserve every support, especially where they set students thinking.

The opportunities for education, even in the higher branches, should be broadened, for no one knows whence genius may arise. It may have its birth with the rich or the poor and, given opportunity, amazing ability will disclose itself from the most unexpected sources. The industries have at last learned the value of pure science, and today the real scientist need never go hungry, nor will he find his contemporaries inclined to quarrel with him. He has a free field. The industries, without any further incentive, will liberally support pure science and the man who can apply discovery. Scientific institutions, like The New York Academy of Sciences, bring together scientists for conferences and crystallization of ideas. These organized scientific bodies perform an extremely useful service. No man of science should fail to join and support his scientific organizations, for in them he will receive the stimulus of association with his fellows. In these organizations his theories will meet with scientific test without controversy, his papers will be discussed constructively, and his opportunities greatly enlarged. No philanthropist can make a better investment for the future of mankind than by supporting scientific education, scientific organizations, and pure scientific research and its applications. He will thus give the future the practical benefactions of pure science.

NEW YORK ACADEMY OF SCIENCES

Address given at Annual Dinner and Meeting

DECEMBER 13, 1939

RICHARD ARCHBOLD, Research Associate in Mammalogy, The American Museum of Natural History, New York, N. Y.: *Flight to the Stone Age.* (This lecture was illustrated by colored motion pictures and lantern slides.)

New Guinea offers a virgin field to the biological scientist. It is still comparatively free from the depredations of modern civilization. It is the last stand of the Stone Age Man. Roads are practically non-existent. The jungle and its flora and fauna may well be more or less as they were in the Pleistocene Era. Here is Natural History in the raw—the material which we hope will eventually supply the missing parts in that jig-saw puzzle which is the science of biology. New Guinea and other such isolated areas may some day offer the clue which will solve the mystery surrounding the beginnings of human life.

It seems ironical that we should still seek the clue to the beginning of human life while the world, in this advanced age, is intensively engaged in the business of ending life. War in Europe is once more threatening the foundations science has laid for civilization. What we have built to benefit mankind is being used to destroy mankind, and those who are serving in the cause of pure science are faced with the need to relinquish their researches or mark time.

Scientists here are also feeling the pressure. At least the war and its threat to our work have already been brought home to me. Because of chaotic international conditions, I've had to postpone my fourth expedition to New Guinea which was to begin in the latter part of 1940. The work that I hoped to finish there must now wait. But I do not propose to mark time. Within a month, members of my New Guinea expedition

will be at work upon life histories of animals in a section of the United States still incompletely studied by biologists.

The situation, as the war affects scientists in Europe, places an added responsibility upon American scientists. It becomes our duty to buttress the foundations science has constructed for civilization. We must double our efforts in all fields of scientific research lest the world we live in revert to the ways of the Stone Age Man as we found him in New Guinea.

Due to the absence of roads in New Guinea, transportation is the problem the explorer there must first solve. As a result of lessons learned on my two previous expeditions to that land of the cannibal and headhunter—the first in 1933 and the second in 1936–37—I chose a twin-motored, U. S. Navy PBY bomber, a flying boat with a cruising range of 4,200 miles, and removed the armament. After satisfactory test flights in the United States and West Indies, we left San Diego, California, on June 2, 1938, and flew by way of Honolulu and Wake Island to the expedition's base camp at Hollandia, Netherlands New Guinea, where we arrived on June 10th.

Waiting for us there and ready for field work were Dr. A. L. Rand, ornithologist; L. J. Brass, botanist; William B. Richardson, mammalogist, and Harold G. Ramm, base radio operator. An entomologist and a forester supplied by the Netherlands Indies government, a detachment of soldiers to protect the expedition from attack by inland tribes, as well as Dyak carriers, were also on hand. The complete personnel of the party numbered nearly 200.

Lake Habbema, 11,500 feet above sea level and about 150 miles from Hollandia, was to be our inland base. Since there was a possibility that our flying boat might not be able to take off at so great an altitude, a group of soldiers and carriers was first flown to a point on the Idenburg River from which they could make contact with Lake Habbema overland in the event that the first airplane party was marooned.

A rescue expedition, however, was not necessary. Throughout the year we remained in the field, we had no difficulty

getting the plane off the lake. Thanks to that good ship, we were able to explore and collect specimens over a wide area of unknown territory. Of the thousands of mammals, birds and plants we brought back, many of them have already proved to be new to biologists. An outstanding result of the expedition was a study made by Dr. Rand of the habits of the bird of paradise in its native habitat. So far as I know, this is the first time a scientist has been able to observe that bird in its own environment.

More spectacular, from the layman's point of view, was our discovery of a tribe which we estimated to number 60,000 and which showed evidence of never having seen a white man before. We found them in an unmapped valley of the Balim River about 3,000 feet above sea level. As we flew in, we noticed gardens ditched, terraced and protected by stone walls, extending for miles. From the air they looked like neat patchwork quilts as a meticulous New England spinster might put one together. We had seen native gardens before, weedy patches in small clearings, but never anything so well kept or on so grand a scale as these.

To our amazement we found they had wrested their gardens from the forest, using stone axes to cut down the trees and heavy wooden sticks wielded by hand in place of plows and cultivators. A closer inspection showed that they were good farmers, raising taro, sweet potatoes and pigs, and seemed to understand irrigation and the prevention of soil erosion.

When we met them they were usually friendly and often greeted us completely unarmed. Oddly enough, in trading with us, they scorned our steel axes and preferred cowrie shells for their pigs, vegetables and fruits. We soon discovered that they were not only good farmers but splendid engineers, knowing many tricks of modern suspension bridge-building, having such a bridge, made of limbs and branches of trees held together with rattan, across the Balim.

Although we did not plan it that way, our flight home also proved to be spectacular. By the time we reached Floyd

Bennett Field in New York we found that we had succeeded in making the first aerial circumnavigation of the globe at its widest diameter. Our route took us across Australia, the Indian Ocean, Africa and the Atlantic Ocean.

HENRY W. SCHOENBORN, New York University: *Studies on the Nutrition of Colorless Euglenoid Flagellates.* (A. Cressy Morrison Prize Winner, 1939.)

This is a fundamental contribution to the study of food requirements of animals. *Astasia* has no chlorophyll, and therefore no photosynthetic ability. Schoenborn has shown that this one-celled animal can be grown indefinitely in a solution of the following simple salts: ammonium phosphate, magnesium sulphate, potassium phosphate, potassium chloride, iron chloride and sodium acetate. *Astasia* requires no carbohydrates, no fats, no proteins, and has no need for any of the vitamins. From such simple chemicals, this animal is able to synthesize all of the substances fundamentally essential to living matter.

Use of the one-celled animals, or Protozoa, for experimental material simplifies tremendously the technical problems of the investigator. These simple animals carry on all the basic phenomena of life in the simplest possible form, without such things as endocrine organs, circulatory systems, digestive systems and other structures to complicate the interpretation of experimental results. In other words, life is here reduced to something approaching its lowest common denominator, to a simplicity which enables the investigator to comprehend at least some of the observed phenomena and even to control them within experimental limits. Such primitive animals can be subjected to rigidly controlled physical and chemical conditions. Furthermore, their food supply can be controlled, not only with regard to kinds of food but even to the exact amounts within a small fraction of a part per million. Under such conditions it is possible to determine exactly what chemical ele-

ments which are essential to living organisms. It also becomes possible to determine whether a need for vitamins and similar substances is a fundamental characteristic of living organisms, or whether such needs have been developed only by the higher organisms in the course of their evolution from simple ancestors. Such experimental material also promises much for the future in determination of the exact changes which animal cells produce in foods, and in the identification of chemical products which the simplest animals eliminate or else store as reserve materials.

DOCTOR BARNUM BROWN, American Museum of Natural History, and DOCTOR ERICH M. SCHLAIKJER, Brooklyn College: *The Structure and Relationships of Protoceratops*. (A. Cressy Morrison Prize Winner, 1939.)

Protoceratops, meaning "first horned face," is the scientific name of a particular kind of dinosaur, whose fossilized skeletons were found in the Cretaceous red-beds of Shabarakh Usu, central Mongolia, by exploring parties under the leadership of Dr. Roy Chapman Andrews in 1922, 1923, and 1925. This was the species of dinosaur that laid the famous dinosaur eggs and the explorers collected and shipped home to the American Museum of Natural History in New York more than sixty splendidly preserved jaws and partial skeletons, nearly a dozen fine skeletons and several nests of the fossilized eggs. It took years of patient and skilled labor to remove the sandstone matrix (originally of wind-blown sand) in which the skeletons were buried and several more years of careful study of the delicate skulls of these animals. The skulls range from those of newly-hatched young just out of the egg, upwards through infancy, childhood and old age, to the largest supposed male skulls, which were about thirty inches long from the tip of the beak to the back of the bony crest on top of the head. The skull was composed of forty-six bones, each of which changed its shape gradually from infancy to old age. There were also

nineteen bones in the lower jaw and the authors describe and figure these and trace their changes from infancy to old age.

On the basis of extensive comparisons the authors conclude that during these individual growth changes the skull bones of *Protoceratops* were becoming more and more like those of the later horned dinosaurs of the Upper Cretaceous of western North America.

Doctor John A. Wheeler, Princeton University: *The Problem of the Scattering of Fast Electrons.* (Honorable Mention, A. Cressy Morrison Prize Contest, 1939.)

A quite serious discrepancy has existed between the observations of the scattering of fast electrons by matter and the accepted theory of the process. It has been thought that it might be necessary to assume that a yet unknown type of force exists in the atoms, and acts on the electrons penetrating them, in order to account for the observed experimental data. However, the author has shown that by taking into consideration the fact that the scattering substance consists of minute crystals that can diffract electrons, much as larger crystals diffract X-rays, it has been possible to develop a theory that accounts for the experimental findings. The result is an important one since it tends to confirm, for magnitudes of the order of the diameter of an atom, the validity of Coulomb's law, *i.e.,* that the force between two electrical charges is proportional to the product of the charges and inversely proportional to the distances between them. A possible cause of complication of existing theories of atomic structure and of interaction between the electron and nucleus has been removed.

NEW MEMBERS

Elected in December, 1939

ACTIVE MEMBERS

Broadbent, B. Holly, D.D.S., Research Associate in Facial Anatomy, School of Medicine, Western Reserve University, Cleveland, Ohio.

Farnell, Frederic James, M.D., Assistant Neurologist, Presbyterian Medical Unit, Columbia University, New York, N. Y.

Furman, Martin A., M.D., Instructor, Otolaryngology, Columbia University, New York, N. Y.

Greenspan, Joseph, Ph.D., Instructor and Lecturer, Chemistry, Brooklyn College, Brooklyn, N. Y.

Iklé, Charles, Archæologist, New York, N. Y.

Kay, G. Marshall, Ph.D., Assistant Professor, Geology, Columbia University, New York, N. Y.

McDonald, Ellice, M.D., C.M., Director, Biochemical Research Foundation of The Franklin Institute, Philadelphia, Pa.

Piatt, Jean, Ph.D., National Research Council Fellow, Department of Anatomy, Columbia University, New York, N. Y.

Rosenow, Edward C., M.D., LL.D., D.Sc., Professor, Experimental Bacteriology, Mayo Foundation, Rochester, Minn.

Rusk, Hester M., A.M., Instructor, Brooklyn Botanic Garden, Brooklyn, N. Y.

Ryan, Thomas J., Teacher, Physiography and Physics, Bushwick High School, Brooklyn, N. Y.

Sayer, Arthur Robert, Ph.D., Tutor, Physics, Brooklyn College, Brooklyn, N. Y.

Schmelkes, Franz C., Ph.D., Biochemistry, Assistant Director, Wallace & Tiernan Co., Belleville, N. J.

Shapiro, Joseph, M.D., Assistant Neurologist, St. Johns Long Island City Clinic and Queens General Hospital, Long Island City, N. Y.

Vik, Esther Stubbs, Ph.D., Director, Field Laboratory and Instructor in Psychology, Child Education Foundation, New York, N. Y.

Walker, Alfred, D.D.S., New York, N. Y.

Washburn, Sherwood L., A.B., Instructor, Anatomy, Columbia University, New York, N. Y.

Wells, F. L., Ph.D., Psychologist, Department of Hygiene, Harvard University, Cambridge, Mass.

Willey, Charles H., Ph.D., Assistant Professor, Biology, New York University, University Heights, New York, N. Y.

ASSOCIATE MEMBERS

D'Aunoy, Rigney, M.D., Professor, Pathology and Bacteriology, School of Medicine, Louisiana State University, New Orleans, La.

Gazin, Charles Lewis, Ph.D., Assistant Curator, Division Vertebrate Paleontology, U. S. National Museum, Washington, D. C.

Kraemer, Elmer O., Ph.D., Research Chemist, Biochemical Research Foundation of The Franklin Institute, Philadelphia, Pa.

Sanigar, Edward B., D.Sc., Research Physical Chemist, Biochemical Research Foundation of The Franklin Institute, Philadelphia, Pa.

Stier, Theodore J. B., Ph.D., Assistant Professor, Physiology, Biological Laboratories, Harvard University, Cambridge, Mass.

Swanson, William W., M.D., Associate Professor, Pediatrics, University of Chicago Medical School, Chicago, Ill.

Wakeham, G., Ph.D., Associate Professor, Chemistry, University of Colorado, Boulder, Colo.

Walters, Fred C., Ph.D., Associate Professor of Psychology, Director Research, University of Puerto Rico, Rio Piedras, P. R.

Ward, J. W., Associate Professor, Zoology, Mississippi State College, State College, Miss.

Weir, Wilbert Walter, Ph.D., Forest Ecologist, U. S. Forest Service, Tucson, Arizona.

Westheimer, Frank H., Ph.D., Instructor, Chemistry, University of Chicago, Chicago, Ill.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 | February, 1940 | No. 4

SECTION OF BIOLOGY[1]

January 8, 1940

Doctor H. Jensen, The Squibb Institute for Medical Research, New Brunswick, N. J.: *Our Present Knowledge of Carbohydrate Metabolism.* (This lecture was illustrated by lantern slides.)[2]

Normal carbohydrate metabolism is dependent upon the proper physiological coordination of various biologically active agents. The different phases of carbohydrate metabolism are intimately linked with that of proteins and fats. Apparently one cannot have abnormal protein or fat metabolism without a disturbance in the utilization of carbohydrates.

It is perhaps advisable to present only a brief description of the main phases of carbohydrate metabolism observed in normal animals. Dextrose (d-glucose) is the sugar which is most efficiently metabolized by the tissues of the organism and other sugars are generally converted into glucose by the liver before they can be utilized. It is well established that

[1] No meetings were held in January by the Sections of Geology and Anthropology.

[2] This lecture was announced under the title "Our Present Knowledge of Diabetes Mellitus."

TRANSACTIONS of the New York Academy of Sciences, Series II, Volume 2, No. 4, February, 1940.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

certain amino acids present in proteins can be converted into carbohydrate by the liver. The possibility of the conversion of fat into carbohydrate, however, has not yet been definitely proven.

Absorbed sugar is either oxidized in the tissues or converted into glycogen in the liver and muscle. Little is known of the various intermediary products of the oxidation of glucose to carbon dioxide and water or of the synthesis and breakdown of liver glycogen. It is evidently a rapid and constantly occurring process. Glucose is burned for energy in the various tissues of the body. Muscle glycogen is oxidized to lactic acid, thus furnishing the energy for muscular activity. Part of the lactic acid is then transformed into glycogen in the liver.

The concentration of glucose in the blood is of importance in supplying the various cells with sugar. The maintenance of a fairly constant blood sugar level depends on the proper balance between the mechanism for disposing of sugar in the blood and that responsible for replenishing it. Glycogen formation in the liver and muscles and in other tissues, together with oxidation and excretion of glucose, disposes of the blood sugar. On the other hand, the liberation of glucose from liver glycogen and the absorption of carbohydrate from ingested food may replenish the decreased amount of sugar in the blood. The efficiency with which these regulatory mechanisms counteract each other depends to a large extent on a normal endocrine balance. Any relative or absolute deficiency or preponderance of certain endocrine secretions may result in either an increase (hyperglycemia) or a decrease (hypoglycemia) of the sugar in the blood.

In the following presentation I shall attempt to discuss briefly our present knowledge of the influence of certain hormones on the metabolism of food stuffs. The exact rôle which each of the different endocrine principles plays in the process of metabolism[3] has not yet definitely been established.

[3] When I speak of metabolism I refer to that of fat, protein and carbohydrate.

Insulin: Insulin, the internal secretion of the pancreas, is formed in the islet tissue of that organ and passes from there into the general circulation. Its presence is indispensable for the normal maintenance of metabolism in mammals. The importance of insulin with regard to metabolism becomes evident upon examination of the physiological disturbances in the body which one observes in the absence of the secretion of insulin (pancreatectomy, diabetes mellitus). The following symptoms have been found to be characteristic:

(1) Pronounced hyperglycemia and glycosuria.

(2) Depletion of the glycogen stores in certain tissues (liver, muscle).

(3) Lowering of the respiratory quotient indicating incomplete combustion of glucose.

(4) Increase in the glucose-nitrogen ratio in the urine probably due to an increase in the conversion of protein into glucose.

(5) Development of acidosis probably caused by imperfect fat metabolism.

Injection of insulin will relieve all these symptoms and reestablish a practically normal metabolism. Administration of insulin also effects specifically the carbohydrate metabolism of the normal organism.

The following functions may be attributed to insulin:

(1) Acceleration of glucose oxidation in the tissues.

(2) Increase in the rate at which glucose is converted to glycogen in the muscle and in other tissues. It is still unsettled whether insulin has a direct influence on the formation of liver glycogen or whether it inhibits hepatic glycogenolysis which is caused by certain other hormones. This inhibitory effect of insulin would enable the liver of the normal animal to retain its glycogen, and would also account for the disappearance of liver glycogen in the depancreatized animal (absence of insulin).

Increase in glucose oxidation and in the rate of glycogen formation probably accounts for the fall in blood sugar observed after insulin injection.

(3) Inhibition of carbohydrate formation in the liver from non-carbohydrate sources. Gluconeogenesis is apparently under the control of certain other endocrine principles.

(4) Prevention of the formation of ketone bodies which are formed as a result of incomplete carbohydrate metabolism.

Decrease in glucose oxidation and increase in hepatic glycogenolysis and gluconeogenesis suggests that hyperglycemia may be due to the following factors:

(a) Deficient supply of insulin.

(b) Liberation, either at a normal or excessive rate, of those principles which play a rôle in glycogenolysis and in gluconeogenesis.

Anterior Pituitary: Knowledge of the complex relationship of the secretion of the anterior pituitary to carbohydrate, protein and fat metabolism is at present still incomplete. It is as yet not known whether any of the anterior pituitary principles exert a direct effect on certain phases of the metabolism of food stuffs or whether this influence is solely mediated through certain other endocrine organs such as the adrenals and thyroids.

That the anterior pituitary is associated in some way with carbohydrate metabolism is shown by the following observations:

(a) Removal of the pituitary gland renders the animal more sensitive to insulin. As compared with normal animals, the glycogen stores of the liver and muscles are more rapidly depleted on fasting. Injection of anterior pituitary extracts will prevent this depletion of the glycogen stores and will render the animal more resistant to insulin.

(b) The symptoms of experimental diabetes (removal of the pancreas) are greatly ameliorated following hypophysectomy. The diabetic symptoms are manifested again upon injection of anterior pituitary extracts into such doubly operated animals.

(c) Recently it has been found that a diabetes-like effect can be produced in normal dogs by the daily injection of in-

creasing amounts of anterior pituitary extracts over a period of several days or weeks. The same effect can be obtained more readily in partially depancreatized animals.

Adrenal Cortex: In general it may be said that the influence of this endocrine organ is similar to that of the anterior pituitary. It is known that the functional integrity of the adrenal cortex is under the control of the anterior pituitary. The effect of the secretion of the adrenal cortex on metabolism is indicated by the following experimental findings:

(a) Adrenalectomized animals show a marked decrease in liver glycogen, particularly on fasting, a lowering in muscle glycogen and blood sugar and an increased sensitivity to insulin. Injection of cortical extracts restores the depleted carbohydrate stores and renders the animal more resistant to insulin.

(b) Removal of the adrenal cortex greatly ameliorates the diabetic symptoms observed in totally depancreatized animals. Administration of cortical extracts causes the reappearance of glycosuria in the adrenalectomized-depancreatized animal.

(c) Injection of cortical extracts into partially depancreatized animals produces an increase in blood sugar and glycosuria. In normal animals, a marked increase in liver glycogen, a slight elevation of the blood sugar level and an increase in nitrogen excretion can be observed.

(d) Depletion of liver glycogen of the fasted hypophysectomized animal can be prevented by the administration of cortical extracts.

(e) An anti-insulin effect can be produced by the injection of either adrenotropic pituitary fractions or of cortical extracts (corticosterone).

The effects of the adrenal cortex on metabolism are strikingly similar to those produced by the anterior pituitary. This has a direct bearing on the question of the participation of the adrenal cortex in the metabolic activity of the anterior pituitary.

The observations on the influence of the anterior pituitary

and the adrenal cortex on metabolism suggest that overfunction of these endocrine organs might sometimes give rise to the symptoms of diabetes mellitus.

Thyroid: It has been known for a long time that the secretion of the thyroid has a decided effect on metabolism. The exact rôle which the secretion of this gland plays on the various phases of metabolism, however, is little understood at the present time.

Conclusions: I have attempted to show that carbohydrate metabolism is the resultant of a well-balanced interaction of various endocrine principles. The changes in metabolism are controlled, on the one hand, by insulin and, on the other hand, by those endocrine secretions (anterior pituitary, adrenal cortex and thyroid) which are apparently antagonistic to the physiological action of the pancreatic hormone. The exact manner in which these endocrine principles divert metabolism in one direction or another is still unknown. Perhaps, one may consider them as mediators in some enzymatic processes concerned with metabolism. The view that normal metabolism is dependent on the physiological coordination of various active agents in the body would seem to offer a reasonable and harmonious interpretation of the various experimental findings on metabolism.

SECTION OF PSYCHOLOGY

JANUARY 15, 1940

PROFESSOR C. J. WARDEN, Columbia University: *The Ability of Monkeys to Use Tools.* (This lecture was illustrated by motion pictures.)

The lecture covers very briefly various aspects of a project which has been in progress since 1935 on this general topic, supported by the Council for Research in the Social Sciences of Columbia University, under the direction of the speaker.

It has been the general impression until fairly recently, that the use of tools (manual instrumentation) is limited to man's closest Primate relatives, the Great Apes (Orang-utans, Gibbons, Chimpanzees, Gorillas). The assumption was that this capacity evolved in a rudimentary form at this level and was passed on to become highly elaborated in man as he evolved toward his present level. More recently, however, a number of workers have reported that monkeys—the next lower Primate level—possess some tool-using ability, as shown by the use of a single string, stick, or rake in securing food placed out of reach. The aim of the present project was to explore this type of ability in Old World monkeys (*Rhesus*) and New World monkeys (*Cebus*) and determine the limits of complexity which such types might reach by training, and also to compare the ability of the two major lines of monkey evolution. The animals were tested in a work cage of suitable size, with bars through which rakes (T-sticks) could be manipulated on a platform outside, on which a food-cup containing one-fourth of a peanut could be placed out of reach.

The first task was to develop the ability to use a single rake effectively and regularly in securing the food, when the rake was placed just in front of the bars with no clear connection with the food-cup beyond. The *Cebus* monkeys were more adept at this than the *Rhesus* monkeys from the start, and became efficient in about half the number of trials re-

quired by the *Rhesus* type. After the use of a single rake had been well mastered, the task was to use a short rake to drag in a longer one, the latter being long enough to reach the food-cup. After this stage was learned, 3 rakes were employed, then 4 rakes, and so on up to an 8-rake set-up. In time, all of the 6 monkeys (3 *Cebus,* 3 *Rhesus*) reached the stage at which 4 rakes were required. Even the 8-rake series was mastered by 2 *Cebus* and 1 *Rhesus* monkey. The tests were stopped at this point because the last rake of the series was now so long that the animals had trouble using it effectively. In fact, it was about twice the length of their bodies and was also quite heavy. It seemed clear to the experimenters that some of the brighter monkeys had generalized the task, in some sense, so that a much longer series of rakes might have been manipulated successfully, except for the hard labor involved.

Some two years later, it was decided to complicate the task by placing the rakes on 2 and 3 platforms, each located on a different side of the cage. This arrangement forced the monkeys to bring rakes from one platform into the cage, turn them with the cross-bar end out, and push them through the bars to the other platforms. They also had to judge the lengths of the rakes so as to use them in the correct order. Moreover, the rakes were placed in a mixed order, as regards length, so as to get away from the routine serial factor in the experiment described above.

Although it had been two years since the monkeys had worked on the single platform task, this was readily reinstated after a few trials in the *Cebus* monkeys. The *Rhesus* type required 300 to 400 trials here. Thus again the *Cebus* show a marked superiority to the *Rhesus*—this time in the *retention* of the habit after the lapse of two years. Both of the *Cebus* monkeys mastered the 8-rake series of the 2-platform set-up, and apparently might have gone much further with more training. One *Rhesus* mastered the 7-rake series here but the other one could not go beyond the 4-rake series.

The best *Rhesus* and the 2 *Cebus* monkeys were then tested on the 3-platform set-up with the following results:—the *Rhesus* failed beyond the 7-rake series; one *Cebus* learned the 11-rake series, and the other, after passing the 8-rake test, was discontinued because of a hand injury. It was felt that these records represent about the limits of ability of the two types of monkeys. The point should be stressed, however, that the use of 7 to 11 rakes in a mixed order on 3 platforms means a fairly well developed ability in intelligent manual instrumentation. We conclude, therefore, that this capacity evolved at least as early as the monkey level in the Primate line, and not at the great ape level as has hitherto been supposed.

It was then decided to see to what extent such habits would be retained following extirpation of one or both frontal lobes (pre-motor area forward). The operations were done by Dr. S. E. Barera of the New York State Psychiatric Institute, and the post-operational tests were made by Dr. Wm. Galt of the Lifwyn Foundation at the Columbia University animal laboratory. This work has just been completed and a full report of the findings cannot be made until the results have been properly analyzed. However, I can make a few general statements as to the main facts, but prefer to offer no interpretations at present. Unilateral extirpation did not appear to disturb the habit of using rakes in series on a single platform. The simpler levels of the 2- and 3-platform habits (up to 3-rake series) were still retained, while the more complex habits were not. Perhaps, continued re-training (post-operational) might have brought about the re-instatement of the latter, in part or in whole. Bilateral extirpation still left the single-platform habit intact or, at most, only slightly reduced at the higher levels. The multiple-platform habits, however, were seriously affected. On the 2-platform set-up, the animals failed beyond the 2-rake level and on the 3-platform set-up, they failed altogether.

The results of the project as a whole indicate the presence

of a simple but definite tool-using capacity in both Old and New World monkeys. Manual instrumentation habits, once learned, are retained with little loss over a period of two years or more—especially in the *Cebus* type. The *Cebus* seems to rate above the *Rhesus* in both learning and retention tests for the sort of task here employed. The simpler habit levels are not disturbed by unilateral and bilateral frontal lobe extirpation, whereas the more complex habits are not retained.

ANNOUNCEMENT OF PUBLICATIONS FOR 1940[1]

The following publications will be issued by the Academy during the current year. Members of the Academy who desire to receive these papers will kindly request the Executive Secretary to send them, and they will be mailed, free of charge, as they are ready for distribution, except as qualified in the footnotes:[2]

ANNALS:

1. "Kinetics in Solution." This article will contain the papers delivered at the conference of the Section of Physics and Chemistry, February 24 and 25, 1939. (About 100 pages.)
2. "Studies on the Nutrition of Colorless Euglenoid Flagellates," by Henry W. Schoenborn. (About 40 pages.)
3. "Studies in Chelonian Osteology; the Fossae and Temporal Arcades of Chelonian Skulls," by Herbert Ruckes. (About 35 pages.)
4. "Dielectrics." This article will contain the papers delivered at the conference of the Section of Physics and Chemistry, April 14 and 15, 1939. (About 100 pages.)
5. "The Distribution of Density Within the Stars," by Henry Norris Russell. (About 15 pages.)
6. "The Structure and Relationships of Protoceratops," by Barnum Brown and Erich M. Schlaikjer. (About 150 pages.)
7. "Free Radicals as Intermediate Steps in the Oxidation of Organic Compounds." This article will contain the papers delivered at the conference of the Section of Physics and Chemistry, November 10 and 11, 1939. (About 100 pages.)

[1] Notice of additional publications to be added to this list will be sent to members later.

[2] Student and Associate Members are entitled to receive one complete monograph or up to 150 pages.

TRANSACTIONS:

Series II, Vol. 2, Nos. 1–8. These papers are sent to all members of the Academy, regularly during the academic year. (About 150–200 pages.)

SCIENTIFIC SURVEY OF PORTO RICO AND THE VIRGIN ISLANDS:[3]

Volume XVIII, Part II: "Porto Rican Archeology," by Froelich G. Rainey. Published February 13, 1940.

Volume XII, Parts 2 and 3: "Insects of Porto Rico and the Virgin Islands—Heterocera," by William Schaus.

[3] The Scientific Survey publications are available to Members for $1.50 per part and to non-members for $2.00 per part.

A certain number of parts, as announced each year, are available free to Honorary, Life and Sustaining Members.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 MARCH, 1940 No. 5

SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 5, 1940

LIEUT. PAUL A. SMITH, U. S. Coast and Geodetic Survey, Washington, D. C.: *Exploring the Continental Shelves and Slopes.* (This lecture was illustrated by lantern slides.)

Knowledge of the topography of the sea floor has grown rapidly during the past decade, largely through the development of echo-sounding. Due to the accompanying need for more complete nautical charts, several of the maritime nations have extended hydrographic surveys seaward from their shores and, due to this seaward extension of the surveys, a whole new physiographic realm is being opened to students of earth science. The perfection and use of offshore position-finding methods and echo-sounding by the U. S. Coast and Geodetic Survey have added many thousands of square miles to the surveys of the continental shelves and slopes of United States and waters under their jurisdiction. The discovery of a stream-erosion type of topography on the seaward faces of the continental shelves under hundreds of fathoms of water has raised questions that seriously affect a number of cherished geological theories. While topographic characteristics of numerous submarine canyons are

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 2, No. 5, March, 1940.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

clearly and unmistakably those commonly observed in valleys eroded by running water above the sea, many geologists have felt that the simple explanation of a lowered sea or rising land in the order of 10,000 feet, in late Tertiary or Pleistocene time, is untenable in the face of apparently well-established theories. A number of hypotheses have been advanced in the attempt to explain these features by some process that may harmonize with other geologic evidence, but the origin of submarine canyons is still controversial.

Through a number of contributors including Dr. H. C. Stetson of the Woods Hole Oceanographic Institution, Dr. Maurice Ewing of Lehigh University, the Carnegie Institution, and collaborators, and Dr. C. S. Piggot of the Geophysical Laboratory, our knowledge of the materials and underlying strata of the sea floor is being expanded. In all of this work, the Geological Society of America has been especially interested, and has contributed greatly to the growth of knowledge in this field of science through grants to various investigators.

Automatically recorded profiles of submarine topography of the Atlantic Continental Slope show decidedly V-shaped cross sections, in the extreme heads of the canyons as well as in depths of 6,000 feet and over. Due to certain limitations of echo-sounding, it is sometimes impossible to record the bottom of steep, narrow ravines in deep water, or to record true slopes in very rugged topography. Graphic records of echo-sounding always give a more complete picture of the profile than visual indicators and, in many instances, reveal phenomena that might otherwise be missed, such as the great sand waves found south of Nantucket Island, during the summer of 1939, by the Coast and Geodetic Survey ship *Oceanographer*.

The abrupt change from the topography of the shelf, which is plainly marine-made in character, to that of the slope which shows no indications of marine erosion, but is plainly a type of topography commonly attributed to stream-erosion,

is one of the most remarkable of the many interesting facts demonstrated by the new surveys. The coöperation of the Geological Society of America through the late Dr. A. C. Veatch and the Coast and Geodetic Survey has resulted in the publication of these new surveys in the form of special charts for use by various investigators.

SECTION OF PSYCHOLOGY

February, 19, 1940

Professor John G. Jenkins, University of Maryland, College Park, Md.: *The Questionnaire as a Research Instrument.**

It is a happy state of affairs that certain psychological problems lend themselves to attack by methods of laboratory experimentation. Although we have had to learn that it is ordinarily vain to talk of single variables in psychological research, it is none the less possible, in many cases, to reduce the number of variables to a point where one may safely speak of an outcome of measured dependability. Where this is possible, psychology need not fall back upon more questionable methods and, indeed, there are those who would insist that psychology has no right to venture beyond the limits of laboratory experimentation.

A somewhat larger group of those who call themselves psychologists has held that this point of view eliminates from the field a great many basic problems of human behavior. Accordingly this group has not hesitated to make use of tests and scales, and has even been willing to employ that methodological scapegoat—the questionnaire. It may be that there are among my listeners tonight those who are irrevocably convinced that the questionnaire is a hopelessly bad tool which should be summarily ejected from the research equipment of the psychologist. To such I can only offer the humble bow of an Untouchable in the presence of a Brahmin. There may be others who would hesitate to use a questionnaire unless evidence of improvement could be adduced. To such I hope to offer words of encouragement. And doubtless there are those among you who have repeatedly made skillful use of questionnaires in the past, are using them ably now, and will continue to employ them on a high plane in the

* This lecture was announced under the title "Psychological Research on Aviation Problems."

future. To you I can offer only apologies for boring you by pitching my discussion on an elementary level and concerning my exposition with matters that have long been familiar to you.

The acceptance of the questionnaire as a research device varies over an impressively wide scale. If it would not be amiss, I should like to suggest that this variation in the professional attitude toward the questionnaire is to some extent a function of the widely varying degree of sophistication regarding the methodological development of this instrument. For a decade I have made it my business to look critically at all questionnaires that have come into my hands and to talk to psychologists regarding their understanding of the preparation, use, and interpretation of the questionnaire. As a result I am convinced that no other research device is employed today on so widely varying a scale of excellence as this one. It is granted that *experimentation* can be sloppy and careless, that *statistics* can be naïve and ill-founded, and that *logic* can be terribly warped by the tyro. Yet all of these are fairly readily spotted in publication, even by psychologists of moderate training. With regard to the questionnaire, however, the situation is different. It is easy to point to men whose experiments, statistics, and logic are above reproach, but who have employed questionnaires apparently in blissful ignorance of the fact that this device must meet certain methodological criteria.

Surely you will not question that—at one extreme of a distribution—we find a questionnaire all but entirely free from systematic virtues. Here we have the all-too-familiar case of the questionnaire formulated on an *a priori* basis in the seclusion of the study with no regard for question-form or question-order, circulated through a sampling defined by expediency and interpreted by a triumphant pointing to those gross totals which were pre-determined by the bias of the inquiry. It is scarcely necessary to add that questionnaires of this type will probably seek information that has already

been made available in much more accurate form by precise quantitative experimentation or will attempt to secure vague generalizations regarding subjects concerning which the respondent cannot possibly have accurate knowledge.

By way of specific example, I present in evidence a questionnaire sent me during the current year with the request that I should have 500 of our students fill it in. It was distributed by an organization notably interested in interfering with the convivial habits of those who regard the ingestion of solutions containing ethyl alcohol as a social good. It may be possible to typify the entire inquiry by quoting the first question which read, "Do you ordinarily behave in a more ethical fashion after drinking large amounts of alcoholic liquor?"

If questionnaires of this sort were circulated only by isolated pressure groups interested in special pleading for a foreordained result, the matter might be of no great concern to any psychologist. The cold fact is, however, that the passing years have been marked by the receipt of a number of questionnaires issued by professional colleagues and marked by an internal organization on a level little higher than that of the interrogatory just mentioned. The speaker feels quite strongly that this situation is to some extent a result of the fact that the improvements of various aspects of questionnaire technique have been made in widely separated fields, and that parochial publication has confined knowledge of these improvements largely to the specific field in which they originated. Once one begins deliberately to search through these various fields for information on refinements of common technique, one finds a truly amazing bulk of material, all of which points in the direction of better control and increased accuracy of outcome.

It is perhaps appropriate to confess that an appreciation of this situation was forced upon me in an odd fashion. Several years ago, I discussed this general thesis with the editor of the *Psychological Bulletin,* who thereupon invited me to

contribute an article which would attempt to bring together from widely separated sources available items of information with regard to the use of the questionnaire as a research instrument. At the time, I must admit, I regarded myself as quite sophisticated in the matter and anticipated that the preparation of this general review would occupy whatever time might be available for the next month or two. I know of no better way to characterize the situation than to remark that, although every spare moment has been devoted to the task for more than three years, the review is only now beginning to take shape in a preliminary draft. Instead of finding, as anticipated, that it was merely necessary to transfer from a card file a digest of materials already covered, events proved that almost every article reread led further afield into sources which had been blandly ignored in a less specifically motivated search through the cognate literature. I should hesitate to boast that the end is even now within sight.

To summarize very briefly what has been forced upon me by a review of this heterogeneously located series of publications, I should like to offer a few categorical statements. These statements may then be elaborated and defended as you sit in judgment upon them. I shall ask only that you attempt to defer decision until the discussion is complete.

The statements which I should like to defend are as follows:

1. The questionnaire may properly be used only for the study of unique events and of attitudes and opinions. In circumstances which set a premium upon economy of time and money, however, it is sometimes defensible to use it for the collection of certain other types of data.

2. No attempt should be made to formulate the questions to be used until the author has had time for an extensive preliminary personal investigation of the field to be studied.

3. No questionnaire should ever be used for actual investigation until it has been extensively pre-tested on samples of the population to be studied in the final research.

4. The formulation of the individual questions and the determination of the physical form of the questionnaire should be carried out in the light of available research-information on the way in which these factors affect the results obtained. The investigator must necessarily bear in mind that the response is a function of the form and the position of the question.

5. Interpretation of results cannot justifiably end with the mere scanning of gross totals. Search for covert content and the use of refractionation are vital necessities for significant interpretation.

6. Interpretation of the results should always be made with the realization that the results obtained apply only to those who responded to the questionnaire. It is never defensible to extrapolate from actual respondents to the full sampling.

7. The study of motivation by the use of direct WHY questions may ordinarily be expected to yield only a rationalized account. It is possible, however, to obtain valuable insights by the use of a program of interlocking indirect questions, especially if the questions are laid out along the time-line of the original response.

Turn now to these statements in order. The first statement obviously contains three parts, each of which should be considered separately.

In pointing out the usefulness of the questionnaire in studying unique events, I am heartily indebted to Truman Kelley[1] and I shall not hesitate to use his weapons in defending myself against those purists who would insist that all events are unique. True it is that no combination of hydrogen and oxygen in a test-tube is ever precisely the same as any other, strictly speaking. But the combination of hydrogen and oxygen in a test-tube has the characteristic property of *repeatability*. Turn to psychological realms, and you find many problems that are repeatable, and hence just as prop-

[1] Kelley, T. L. *Scientific Method,* 1–38. Macmillan. 1932.

erly subjectable to experimental investigation as the synthesis of water. We may run the memory drum over and over for many subjects; we may repeatedly induce nervous breakdowns in rats; and we may photograph eye-movements hundreds of thousands of times, if necessary. These *repeatable* events are the natural subjects of experiment and there is no adequate reason to attempt to study them by means of any instrument which involves a larger probable error. Thus, the probable error in any laboratory study of the effects of alcohol is large enough to dismay anyone. To attempt to gauge this effect by means of direct questions increases the probable error to astronomical proportions.

On the other hand, human behavior abounds in unique events. It would appear that psychology must either refuse to consider them—and there are those who would do that—or accept some method which, in spite of a larger probable error, casts some faint light on the situation. You may either ignore the fact that I spanked my son last week, or you may ask about it. That particular spanking-situation is not repeatable. You may pay no attention to the man taking his first airplane flight, or you may ask questions about it. You may close your eyes to problems raised by the loss of a job, or you may ask a jobless man about it. It is the thesis of this paper that social psychology, for example, need not deny itself materials of this sort, provided it understands that the accuracy of the information obtained is a function of the refinement of the instrument employed.

The situation is the same with regard to obtaining information regarding opinions and attitudes. If you hold firm to the idea that opinions and attitudes are not important for psychology, or if you believe that *directly observable* behavior holds adequate representation of opinions and attitudes, then the problem of the questionnaire need not bother you. If, however, you doubt that the situation is so comfortingly simple, then, to obtain the information you need, you must ask questions. Questions may show that a man who votes

the straight ticket does so under political pressure and holds attitudes quite at variance with his voting behavior. Inquiry may reveal that the professor who acquiesces in faculty meeting actually conceals the storms within his soul. Questions may or may not reveal what is concealed. We shall presently suggest that *direct* questions will probably lift few curtains but that a program of *indirect* questions, formulated in the light of modern knowledge of methodology, may open doors so gently that the respondent himself is unaware of revealing anything.

Finally, although the privilege is often abused, the questionnaire may be used to obtain certain types of factual data when the situation sets a premium upon economy of time or money. It would be possible, for example, to travel and to learn by direct observation how many hours per week psychologists are required to teach, but the cost would be exorbitant. Adequate results can be obtained by a properly sanctioned questionnaire. Again, it would be possible, by direct observation, to determine the laboratory equipment of the 40 largest laboratories, but again the expense involved justifies the use of a properly sanctioned questionnaire.

Now, nothing that has been said is intended to deny or to condone the abuses that all too often characterize the use of the questionnaire, even within the fields named. There is little reason to believe that defensible results can be obtained by inquiring about unique events or attitudes that lie far in the past. I shared the impatience of a 65-year-old colleague when he was asked what his interests had been at the age of 5.[2] Likewise there is no defense for questions which seek information not actually at the command of the respondent.

[2] At the meeting at which this paper was presented, J. C. Flanagan reported that Rulon had obtained evidence of high accuracy when he asked members of a 25-year class to report on their interests as undergraduates. The reference is not available in standard psychological sources. Even such a demonstration would be highly specific in nature and in time; and, in view of all that has been brought out in psychological studies of testimony, the burden of proof would always rest with those who would assume the accuracy of inquiries about the long past.

Only recently I was asked to estimate the amount spent annually on liquor by the average undergraduate at my university. If I had responded, the meaningless numbers I set down would doubtless have been thrown with others and averaged to several decimal places. This is a species of professional malfeasance that is below contempt. And we should have only severe censure for those who lazily attempt by questionnaire to obtain information more accurately available in other sources. There is no reason to estimate by questionnaire the percentage of APA members holding the Doctorate when the annual directory readily supplies an accurate answer.

It would appear that the first rule of questionnaire construction should be to study the appropriateness of the method to the problem at hand.

If you will grant at least the major emphasis of the first statement, we may turn to consider the second. This, you will recall, read about as follows: "Only after adequate time has been allotted for a preliminary personal investigation of the field under consideration should the questionnaire be formulated." Here is a point on which the divergence between excellent and poor questionnaires is wide indeed. A certain Ivory Tower tradition has led to the growth of the idea that the psychologist, being a capped and gowned Prober of the Mysteries, needs only the esoteric activities of the Little Gray Cells in order to turn out an effective questionnaire in any field. (Would it be impertinent to remark that the present generation of psychologists shows much less of a tendency to don the long robe and to play the savant? Or have you forgotten the academic atmosphere which led an American psychologist to write, three decades ago, in terms of a Doctor Watson in the form of a layman who always marvelled at things mysterious to him which were as an open book to the trained psychologist?)

The field of psychological market research has played a dominant rôle in showing the importance of the informal in-

vestigation which precedes the formulation of the first question. We have many trained psychologists in this field today and none would venture to make up an arm-chair questionnaire. They have had to learn that a brief trip into the field is always necessary to find out what to ask, how to ask it, what words to use, what questions will embarrass the respondent—in brief, to get the "feel of the situation." In this preliminary investigation, too, one frequently sets up hypotheses which serve to shape the whole course of the investigation. Leave out this stage and you will surely find yourself with questions which are unnecessary, questions which are worded improperly, set in terms which are either obsolete or ambiguous, and you will quite likely find that your beautifully spun hypotheses—which looked so fine in the quiet of your study—are pitifully inadequate in a world of real men and real events.

Let me offer an example or two from the field of marketing research. How was a city boy—even though a psychologist—to know that a study of material for admitting ultraviolet rays to hen-houses would necessitate a question as to whether the windows were washed and, if so, how often? Or, how would the same city boy know without preliminary study that most of his respondents would think that the word "peer" meant *superior?* Or, if you were invited to study why Community Chest returns fell off in a given city, how would you know what questions to ask, until you had chatted with the local folk to get some leads? Or, if you were sent to discover what laborers in the steel mills thought of unions, how could you possibly phrase your questions for them without preliminary study right among the steel workers?

Fortunately, signs of using the preliminary investigation are not limited to the technological fields. Some of you doubtless remember a paper given at Minneapolis by Augusta Jameson[3] in a study of the development of delinquency in

[3] Jameson, Augusta. Psychological Factors Contributing to the Delinquency of Girls. *Jour. Juven. Res.* 22: 25–32. 1938.

girls. If so, you will recall that she first went to the institution in which the study was made and spent several months—not days—simply acquainting herself with the girls and with the world in which they lived. Only after her stay there had been long enough to gain her acceptance into the group, did she begin to ask their aid in phrasing her questions. The final questionnaire, phrased in terms designated by the girls themselves, and touching upon matters of importance discernible only under direct investigation, could be relied upon to gain information that would never have been obtained by an instrument phrased in collegiate terms and aimed at matters assumed *a priori* as important by an academician.

It is obvious that this second statement leads naturally over into a third, which reads: "No questionnaire should ever be used for the actual investigation until it has been extensively pre-tested on samples of the population to be investigated." Now the man who will design a questionnaire in the silence of his study, will usually disdain any field test—yet even the remotest acquaintance with the results of such a test cannot fail to carry conviction. In actual practice, the pre-testing of a questionnaire corresponds closely to the period of experimentation known colloquially as "getting the bugs out of the apparatus." As a result, it is not uncommon to spend months pre-testing a questionnaire which may achieve its final usefulness in a few weeks or even days.

The series of pre-tests will detect ambiguous and unclear questions. It will show unforeseen effects of one question upon another and indicate necessary changes in question order. Almost inevitably it will show that certain questions are unnecessary and that other important questions have been omitted. It will indicate changes in wording and changes in methods of recording the answers. In fact, it will most embarrassingly show one how feebly the human intellect can function in preparing an *a priori* analysis of a complex situation. In the field of market research, a series of pre-tests has become mandatory and the bulk of evidence on its effects

has become monumental. In certain fields of socio-psychological investigation, alas, it is almost unknown.

You will appreciate that it is difficult to bring in specific evidence on this point, except by way of example. Very recently, for example, our staff took part in a study of the sale of photographic materials. Seeking only to study the acceptance of polychromatic pictures, we asked whether the respondent took colored pictures more often than plain ones. Looking back now, I see readily that certain negro respondents might have taken the word "colored" to refer to ethnological rather than to chromatic differences and have responded accordingly. I did not see it in advance, but a pre-test brought it out. A pre-test also showed that most of our poorest respondents thought that the General Electric Company generated the power for their lights at home. Another pre-test revealed that college freshmen were quite unable, without personal assistance, to answer the question: "In what circumstances did you reach the final decision as to what college to attend?" (We were forced to grant the palm to one student who answered, quite justifiably, "In very poor circumstances.")

In a word, the questionnaire, as it comes from the platen of the author's typewriter, may be clear, unambiguous, unbiased, and unembarrassing only to the author. Preparing it for effective work in the field means pre-testing it—not with available and long-suffering graduate students—but in the field and with a sampling of the final population. Such a pre-test inevitably leads to revisions which must then be subjected to a further pre-testing. In the speaker's experience it is a rare questionnaire which will be ready for use with less than a half dozen revisions and pre-tests.

Pre-testing points to the necessity for phrasing the questions in terms of the requirements of the respondent. Our fourth statement suggests that wording and physical form must be aligned also with rules developed in the course of psychological research. The study of the effect of the form

of a question was not new when Muscio[4] worked experimentally on it more than two decades ago or when Burtt and Gaskill[5] tackled it in the laboratory some time later. The mere inclusion of a definite article has been shown to influence the response. Likewise the change from a specific to a general form has been shown to have noteworthy effects. More recently, we have seen evidence that the inclusion of a famous name as the sponsor of an idea will materially change its acceptance. And we have seen that the attaching of a label may alter the very sign of the response, as when those who indignantly reject a doctrine labelled as fascistic, enthusiastically embrace it when presented without any label. He who formulates questions only stores up trouble for himself if he works in ignorance of these elementary facts.

Go beyond these basic principles and you come to many other available guides for the shaping of questions. Ghiselli[6] has shown that providing for four possible responses, instead of a simple yes–no alternative, may alter not only the proportion, but, as well, the actual balance of opinion. This is one of many links in the chain that would restrict the use of yes–no questions to situations in which one has to choose only between mutually exclusive alternatives. Much work that has been done on rating scales and testing techniques may profitably be carried over as a bulwark against those who would unjustifiably make of the yes–no question a veritable Procrustean bed in which human attitudes are trimmed to the taste of the interrogator.

The yes–no type of question, one suspects, is introduced largely because it lends itself so conveniently to scoring. Very likely the same motive has led to the popularity of the multiple-choice or check-list type of response. The speaker

[4] Muscio, B. Influence of the Form of a Question. *Brit. Jour. Psychol.* 8: 351. 1916.

[5] Burtt, H. E., and Gaskill, H. V. Effect of the Form of the Question. *Jour. Appl. Psychol.* 16: 358. 1932.

[6] Ghiselli, E. E. All or None versus Graded Response Questionnaires. *Jour. Appl. Psychol.* 23: 405–413. 1939.

began to suspect this form when a result encountered in a consulting capacity carried with it an odor that was unmistakably piscatorial. An effort was then made to determine possible results in the event that the list of items was not exhaustive when presented to the respondents. Some of you may recall that a test[7] was made with several hundred students. One group was given a free-response question and, on the basis of frequency of their replies, a check-list was made up. Although the order of items was scrambled, the values received on the check-list coincided closely with those on the free-response. Then the check-list was mutilated by the deletion of the five most popular answers and the list was presented to a third group, with the request that they should supply any items they wished. No longer were the results similar. The other items moved well up on the list and the five original top items were barely mentioned. The general result, I may say, has since been verified in quite a different setting.

Such studies have led to the formulation of a general principle. A check-list is satisfactory for the comparison of a limited number of known possibilities, but it should not be used to explore a field in which all of the possible answers may not be represented on the list.

The quest for "objectivity" has led to the use of two other mechanical devices—ranking and rating. The potential evils of the first are so apparent as to require little comment. Any attempt to average ranks assigned to the several answers implies a tacit assumption that the ranks are separated by equal psychological distances—an assumption rarely if ever justifiable. It involves one in the mathematical sin of treating ordinal numbers as if they were cardinal numbers—a sin encountered in the litany of grade-school and, unfortunately, early forgotten. The speaker was once in a position to demonstrate to a commercial group what this meant, for the

[7] This has never appeared in the periodical literature but is described in: Jenkins, J. G. *Psychology in Business and Industry.* 348–351. Wiley. 1935.

respondents had been forced to comment on the meaning of their rankings. These comments showed clearly that for one respondent, No. 1 and No. 2 were almost ties, while, for another, No. 1 was infinitely far above No. 2—and so on. Yet in the report where these figures were presented, these ranks had been solemnly averaged to the second decimal place. Where the proponents of the device have sought to use it for ease of interpretation, the speaker would say that one should avoid the use of ranking in view of the practical impossibility of telling what the ranks mean.

Nor does rating fare much better, when critically regarded. Quantitative estimates of this sort may be easy to give, but their validity is dubious to the extreme. Within the past ten days the speaker has been asked to pass upon a questionnaire in which he was called upon to rate the percentage of time in which he was characterized by each of a large number of adjectives. The author of the device insisted that any intelligent adult should be able to estimate, within a small margin, whether he had been "honest" let us say, 80 per cent of the time or 70 per cent of the time since birth. If this be so, I must confess that I fall short on both intelligence and adulthood. In any event, the author, in claiming high validity for his instrument, based his claims of validity upon the fact that undergraduates found it easier to fill out than any other form. This, I take it, is validation by expediency—a new methodological concept!

When evidence on question-forms is examined, we are faced with this general situation. The use of yes–no questions is approved when all that is asked is a choice between mutually exclusive alternatives. The multiple-choice form is approved when evidence is shown that all possible choices are represented. Doubt is cast upon the desirability of rating and ranking. This means that, in most cases, the free-response form is indicated, and this, indeed, must be formulated in the light of all we know about how the question influences the answer. Free-response questions may be inconvenient

to tabulate, but, if properly worded, they do not warp the response by their own mechanics.

The study of free-response questions includes, of course, all of the materials available from the many studies of the psychology of testimony. It must be expanded as well to include a search for questions that are "loaded." A questionnaire recently laid in my hands asks: "Do you enjoy the luxurious ease and personalized service of travel by air?" It involves as well a search for questions that are perfunctory, that put a premium upon dishonesty, or that require answers interdicted by social usage. It involves a realization that questions that would be embarrassing must be rephrased and that a series of indirect questions may succeed where a single, direct question would fail. And, above all, it involves a certain knowledge that asking the same question from different angles permits a series of checks upon internal consistency that would otherwise be lacking.

Finally, of course, the formulation of questions rests upon the realization that the respondent always answers a question-in-a-setting and never the question-as-phrased-by-the-author.[8] Chesterton pointed this out in one of his Father Brown stories many years ago, and anyone who has worked in the field with actual respondents has encountered it many times. Thus, if I ask one of you, "Are you doing anything after the lecture tonight?" you may say "No," ignoring the fact that you plan to take the subway, go home, order the milk for tomorrow, read a short story, and then revise six pages of an article while trying to go to sleep. Your "No" refers to activities which fit into the context of my probable invitation to share in a discussion at a nearby restaurant. It ignores the routine activities in which I probably have no interest. There is perhaps no better indication of the truth of this statement, than the social rating we assign to the intolerable bore who

[8] This is one of several pertinent points treated in: Roslow, S., and Blankenship, A. B. Phrasing the Question in Consumer Research. *Jour. Appl. Psychol.* 23: 612–622. 1939.

insists on replying literally and in detail to our polite inquiry "How are you today?"

In our social life of everyday this tendency may be a life-saver. In questionnaire studies it poses a very difficult problem. Lazarsfeld[9] has pointed out that many respondents approach a question with some *tacit presupposition,* and that this presupposition may vary from person to person. Realization of this means that we must bend every effort in formulation and in pre-testing to discover and to care for possible contexts which may cause widely differing interpretations of our questions. No general rule is possible. Much hard work in the field may, however, go a long way toward securing a uniform interpretation of the question as asked.

If recent years have brought improvements in the form of the questions used, they have brought even greater possibilities of improvement in the techniques of interpretation. It is not difficult to turn back only a decade and to find many examples of questionnaires which were skillfully constructed, distributed to an adequate sampling, and then set into publication with no interpretation beyond the gross scanning of totals. The well-trained specialist in this field today knows that his work is only well begun when the totals of the several columns are available. There are two major tasks which lie before him when the cold figures are finally available. The first of these is the search of the data for covert content indicative of a theory or principle which may in itself immediately require further investigation. The second is a patient process of fractionation and re-fractionation of the raw data in search of relationships which, although otherwise concealed, may well provide the most important outcomes of the investigation. Let us consider these in turn.

We may illustrate the first by turning to an extremely important publication which has just come from the Fatigue Laboratory of Harvard University. As most of you know,

[9] Lazarsfeld, P. F. The Art of Asking WHY in Marketing Research. *Nat. Marketing Rev.* 1: 1–13. 1935.

Elton Mayo and his associates have devoted a decade of intensive study to certain aspects of the personnel situation in a plant of the Western Electric Company. (The study began, you will recall, when a more or less routine investigation of the effects of rest-pauses failed to obtain the results that were expected.) In the most recent report of this long-time study, Roethlisberger and Dickson[10] describe the application of the questionnaire technique to a study of the complaints of a large number of employees. A few years ago, I suspect that the end of the study would have come in the form of a frequency-tabulation of the complaints made by the workers. The results, however, compelled these investigators to adopt the point of view that such a tabulation would have little meaning —that is, that the actual items complained of were not in themselves significant causes of difficulty. This led in turn to a detailed consideration of the social organization informally and undeliberately set up by the workers and showed that offenses against this social structure were the real causes of unrest. (In passing, I am impelled to say that, if this book is not an important highlight in social psychology and in labor economics, it will be for reasons beyond the speaker's present comprehension.)

The example, at any rate, is clear-cut. Take the frequency tabulation in itself, rectify the items complained of, and you have failed to change the situation significantly. The tabulation takes on meaning only as it is seen against the background of a unifying theory provided by the covert content of the responses. The same idea was well expressed by a visiting psychologist who once told one of my classes: "Professor Jenkins has shown you how beautiful tables and charts may hide the real facts. He has shown you how to make analytical tables and charts. Now I tell you, throw them all away and make instead a theory." (And to make his point, he showed the class a beautifully unifying theory for the very

[10] Roethlisberger, F. J., and Dickson, W. J. Management and the Worker. 255–269. Harvard Univ. Press. 1939.

data over which we were working and which I, in my enthusiasm for mere refractionation, had overlooked.)

It will be clear to all of you that such theories are not to be built by idle speculation—that they must be *empirical* theories. Now, one of the greatest aids in building up an empirical theory is precisely the fractionation and re-fractionation already mentioned. On an early consulting job, for example, I was faced with beautifully drawn graphs which showed that the sponsoring company was obtaining a very large share of the market for a group of products. The conclusion to be drawn from such graphs was unmistakable—the current policy of the company was above reproach. Fortunately the raw data were available and we began to re-fractionate them. The very first check showed that, although the company was selling more than its expected share to very large buyers, it was selling much less than its expectancy to small buyers. A second fractionation showed that the geographical distribution of sales was by no means uniform. And a third indicated that sales varied considerably with the nature of the mechanism for purchasing. Instead of a confirmation of the existing policy, we were faced with a very much more complex interpretation of the situation.

In market research situations this is so generally the rule that no research group today would venture to stop at the scanning of gross totals. And it is not difficult to show that the same situation obtains in many other fields of research. In one study of college-attendance, for example, the gross totals themselves would have indicated conclusions which were greatly at variance with those obtainable when the results had been successively broken down by economic level, by geographic origin, by college registration, and by types of institutions considered. Likewise a recent study of faculty attitudes with regard to establishment of academic policy would have fallen far short of the mark, if only totals had been considered. Fractionation of the results indicated a wide disparity between certain well-defined groups indicated

by breakdowns according to age, to academic rank, and to length of service. The process of fractionation is vastly aided if the material is coded and punched onto cards for machine sorting, but the difficulties of hand sorting may be well justified by the results obtained where machines are not available.

The process of interpretation involves us at once in the whole theory of sampling and in the problem of how far we may go in extrapolating from our sample to a larger universe. I shall not bore you by going into the mechanics of obtaining representativeness and reliability. Suffice it to say that anyone of serious intent can estimate in advance the size of sample he will need to obtain results within a given region of tolerance and then check it progressively in actual application. Sampling theory and practice are well enough developed today so that there is no longer any excuse for some of the gross inaccuracies that are observable in the distribution of current questionnaires.

It is not difficult to show that many who hew to the line with regard to the representativeness and reliability of their sampling blandly ignore the more important problem of whether they are justified in extrapolating from those who *do* respond to those who *do not* respond. Relatively little direct evidence is available on this point, but the evidence that is in, definitely suggests that the burden of proof rests on those who assume that the addition of responses from those who did not answer would not have changed the picture. This problem arises not only in mail questionnaires but in face-to-face inquiries as well. One study of the former type of inquiry was made by W. J. Reilly[11], who conducted interviews with all of those who had received a questionnaire sent out by an advertising agency. A comparison of replies made by those who had returned the questionnaire with those who had not indicated that, on every matter considered, the responding group was not typical of the entire sampling. Most

[11] Reilly, W. J. Checking upon Data Secured by Mail Questionnaires. Printers Ink. 71. Dec. 23, 1926.

important, perhaps, was his discovery that the responding group was definitely more favorable to the product than the group that did not respond. A similar study involving a very careful and persistent effort to cover every potential respondent in two cities yielded results that indicated an even greater disparity between the results obtained from those who responded and those obtainable from the total sampling.

Although the results will always be specific to the situation, many other studies of this sort are needed. If, for example, it is found that the general trend of those who do respond is more favorable than of those who do not, some allowances may be made in interpreting later results. If, as certain findings suggest, the results of those who do respond are more accurate than those which would have been obtained from the total sampling, the fact is surely not without significance. If the ready respondents generally show a greater homogeneity, we should be able to discover this; and if there are characteristic differences from field to field of investigation, the more rapidly the facts can be uncovered, the more we can do to reduce our probable errors. In the meantime, it seems highly necessary to bear in mind that, unless a 100 per cent response is obtained, the results can properly be regarded as shedding light only on those who did respond.

My final—and to me the most significant—statement has to do with the study of motivation by means of questionnaires. I am sure that I represented only the common trend in the field when I told my students 15 years ago that the questionnaire was utterly useless for the study of motivation. It was easy to argue that inquiring about motives yielded only rationalizations, that it obtained "good" motives instead of "real" motives, and that it gave us, at best, only someone's opinion of the forces which motivated him. It was easy to hold up horrible examples, to demonstrate that inquiry reduced the splendid complexity of human motivation to an artificial simplicity. By way of example, I recall, I adduced a popular list of "basic motives" which had been made up by asking student groups to indicate which abstract names of

motives were strongest for them. (Incidentally, I still think this list is as bad as ever I did, although it is still occasionally reproduced even now as an "experimental" study of motives.)

My conversion came about as a result of repeated demonstrations at the hands of Lazarsfeld, although I found later that P. M. Symonds[12] had been insisting on the same general principles for several years—and I presume *he* borrowed them from Aristotle! At any rate, Lazarsfeld insisted, in speech and in writing, that "*Why* is never a question; *why* is always a program"—and he began to produce evidence in support of this thesis. Put into other terms, he advised a study of human motivation by a series of interlocking and indirect questions which should, so far as possible, reproduce the time-line of the original behavior. Thus, the direct inquiry "Why did you come to this lecture tonight?" would, in all probability, elicit a series of nice, socially acceptable, and carefully rationalized responses. If taken at face value, these would leave us with an over-intellectualized view of the behavior involved, couched in terms of desire for self-improvement, interest in methods of the social sciences, plus perhaps a certain curiosity aroused by the idea that this talk was to deal with psychological problems in aviation.

Now suppose we take a much longer route and approach the problem indirectly. We may begin by asking what other lectures of the series you have attended; what others you plan to attend; whether attendance was required by registration in some course; what you usually do on Monday evenings; when you first began to attend psychological lectures and under what circumstances; in what company you came tonight—and so on. If the series of questions is well planned, there may begin to emerge certain characteristic patterns of behavior which cast a good deal of light on what would otherwise be regarded as a virtuous effort at self-improvement.

Thus, in a somewhat analogous study of attendance at the movies, it was found that certain members of the audience

[12] Symonds, P. M. Diagnosing Personality and Conduct. 122–173. Appleton-Century. 1931.

were night-habituated; that is, they always attended on Tuesday night. Another group were star-followers, who made an intellectual virtue of seeing Mae West in all of her dramatic triumphs. A third group represented an escape-from-home pattern, selecting the movies as a convenient way of satisfying an urge to remove themselves from the domestic scene. A fourth group were present under the urge to witness a specific film. A fifth group found in the movies a convenient way of wasting requisite amounts of time between other activities. Those of you who are regular attendants yourselves need not be reminded that a sizable group gave evidence that the moving picture theatre provided for them a surrogate for the balcony scene.

This grouping of motives is by no means the only outcome that may be expected from properly conducted studies. One investigation, for example, showed that a major factor in the cessation of milk-drinking was the identification of milk by the adolescent as a baby-food. Another has sketched clearly the variety of motives effective in book-buying. Others have depicted motivational factors at work in delinquency, in the maintenance of industrial morale, and in the use of campus recreational facilities—to name only a few.

If you object that this technique is not new, I shall be forced to agree. The new thing about it, however, is its conscious application. A psychiatrist, with whom I have had extended conversations of late, informs me that members of his profession have always used the general technique of indirect questioning along the time line, but that, in his opinion, many have failed to realize exactly what they have been trying to do. What is novel to many people, I find, is the idea that the question "Why?" may best be asked by asking a series of questions beginning with the words "What?" and "When?" and "How?" Nor does it take much imagination to see that many respondents who would resist the direct inquiry "Why?" are quite willing to supply the desired information—without ever realizing what they are doing—if the more indirect form is employed.

It is obvious that my half-dozen statements have left unsaid far more than they have mentioned. I have spoken as if mail and face-to-face inquiries were the same, while it is clear they are not. Each has its peculiar field of usefulness and each its peculiar advantages. No mention has been made of the whole problem of motivating the respondent to reply, although much has been written on this topic. I may mention that H. G. Weaver[13] reported a 50 per cent increase in returns when he enclosed a duplicate copy of a questionnaire, and that he obtained more than three times as many returns when he told respondents that they did not need to bother to return the blanks at all. Nothing has been said about the difference between interviewers who report the gist of what respondents say and those who actually report *verbatim* answers—yet the difference in value to the investigator is enormous. Nor has any attention been paid to a most crucial point—that of training interviewers to record "incidental" remarks by respondents. Yet experience has shown that insight into many a problem has depended upon some chance remark not called for by the formal interrogatory. And so we might go on through a score of other points which bear directly upon the formulation and use of effective questionnaires.

In closing, may I say that I hope the technological nature of my examples has not closed the ears of any one in the audience to the principles involved. The selection of the examples was dictated by the activities with which I have been concerned. The applicability of the principles in non-technological fields is above question. The questionnaire will always bring in results involving a certain probable error. In the hands of those who are willing to use it as a rifle, rather than as a blunderbuss, however, it can provide valuable information about human behavior that must otherwise be denied to those who would have it. The speaker hopes that he may have made certain converts towards membership in the Rifle Club.

[13] Weaver, H. G. Consumer Questionnaire Technique. *Amer. Marketing Jour.* 1: 115. 1934.

SECTION OF ANTHROPOLOGY

February 26, 1940

Doctor Jack Harris, Columbia University, New York City: *The Position of Women in a Nigerian Society.*[1]

In West Africa are located the great native kingdoms of Ashanti, Yoruba, Dahomey and Benin. Here the political organization is highly formalized. There are a number of groups bordering on these which, while less formal in their political organization, share many traits of culture with these kingdoms.

The Ibo of southeastern Nigeria form one such group. Approximately 500 politically independent communities roughly similar to Ozuitem, the Ibo community in which the author conducted his research, comprise the Ibo-speaking peoples.[2]

It is a common characteristic of these West African peoples that even when they are formally organized along patrilineal lines the power and importance of women are emphasized. In the great kingdoms their position has been formalized in such institutions as the court of the queen mother and in such formal rôles as those of priestesses, "Amazons," the chief's wife and the queen mother.

Where the patrilineal pattern of patrilocal residence, bride price, secret societies from which women are excluded, and a preference of sons over daughters obtains, as among the Ibo, less formal but equally effective mechanisms for the expression of women's importance prevail.

[1] A number of references to the position of women in Ibo society may be found in Meek, C. K. *Law and Authority in a Nigerian Tribe.* London: Oxford University Press, 1937. However, since Meek's inquiries occurred immediately after the Aba Riots he found it difficult to make systematic or intensive inquiries on this point. See also Leith-Ross, S. *African Women.* London: Faber and Faber, 1939, for personal experiences and general statements concerning Ibo women.

[2] Field work in Nigeria during 1938–39 was made possible through a Fellowship awarded the author by the Social Science Research Council.

Ozuitem has a population of approximately 5500 people. It is considered a social entity because of descent from a common ancestor, sacrifice at common shrines, similar customs and group meetings at time of crises. Most Ibo communities are composed of a number of patrilineal sibs. There are 17 in Ozuitem.

The community is divided and sub-divided into a number of groupings in the following manner:

1. *The community.* All members count descent from a common ancestor.
2. *Sib Groups.* There are three of these in Ozuitem, but for practical purposes of sacrifice, warfare and communal work, two closely related groupings combine, so that there is a working dual division common to all Ibo social units.
3. *Sib,* composed of from one to twelve kindreds—approximately 100 to 1200 people—who count descent from a common ancestor.
4. *Kindred,* consisting of from one to five extended families, who have a more immediate common ancestor than that of the sib.
5. *Extended family* includes from one to about fifteen primary families, who have common descent from an ancestor more recent than that of the kindred.
6. *Primary family,* consisting of a man, his wife or wives and their children.

Each of these groupings has its own executive council. The community council draws its representatives from the sib groups and the sibs. The sib council draws its representatives from the kindreds, the kindred from the extended families, and the extended family council is composed of the adult males in each primary family.

Although men repeatedly emphasize that women are practically chattels, that they have no power other than that allowed them by the men, women have a system of courts and councils which directly parallels those of the men. These are organized on the basis of the kinship units of the women's husbands following the pattern of patrilocal residence.

Women married into one extended family, kindred or sib form groups which pass laws regulating the activities of women, organize their own work, conduct their own courts, impose and collect fines. There also are meetings of women on a community basis at times when their welfare is threatened as, for example, when the men's council passes a law detrimental to the interests of women, or when excessive rainfall is ruining their crops and remedial measures have to be considered. Rare combined meetings of men and women are also held during crisis situations which demand the participation of both sexes as, for example, when a major deity who influences the entire community demands sacrifice and the women's suggestions and financial contributions are as welcome as those of the men.

Throughout the assertion of their rights, implicit in most of their actions, and at the root of their solidarity and coloring their attitudes, is the stress women place upon their rôles in the society as food producers and child bearers. At certain times, this emphasis reaches the conscious level. To illustrate, during the Aba Riots (to be described later), the women were indignant that they, "the trees which bear fruit," were to be taxed. They proclaimed that they felt safe in marching upon the white officials, because they were women and women were mothers—mothers not only of black people but of white. How could the white men harm mothers?

This attitude is also explicit in the form of the curse employed by a woman striking a pestle on the fireplace where she cooks food. She mentions the name of the man who has wronged her, calls upon the female ancestors and continues: "This, that you have done to me, is not good. I did no harm to your mother nor to your people. It is I that bore you in my womb. It is I who gave birth to you. It is I who cook for you to eat. This is the pestle I use to pound yams and coco yams for you to eat. May you soon die!" This phrasing of the curse is general for all women, even for one who has not given birth. The woman assumes the rôle of Ibo

womanhood and is, in effect, saying: "It is we women who give birth to men. It is we women who feed men. How dare you do an injustice to a woman?"

Men and women share responsibility for feeding the family, but upon women falls the burden for a longer period of the year. Women grow 20 crops on their farms and are responsible for the feeding of their families for approximately eight months of the year. From September to the middle of December—from the harvest of yams to the harvest of coco yams—it is the formal custom for men to feed their families. But men grow only one crop, yams, and, during his period to feed the family, a man is often forced to rely upon women's crops, which he frequently must buy in the market from other women, even though his wife may have these crops stored at home. In theory, food responsibilities of husband and wife are mutually exclusive, but most Ozuitem couples coöperate. Yet the theory can be sharply invoked if the couple quarrel. The wife may cut off her husband's food supply and he may later retaliate by withholding hers during the formal periods or by refusing to give her land for the next farming season.

Most men admit that in these situations they are at a disadvantage for a number of reasons. If the husband refuses to supply yams and other food, the wife has a number of crops on her farm or store to which she can turn, but during the women's feeding period, men's one crop, yams, is still in the ground. To meet this exigency many men cultivate a plot of land devoted solely to women's crops. This is called "farm of man who has no mother." The name derives from the fact that it is usually to his mother that a man turns, or, lacking these expedients, his brother's wife or his mistress. He has one other alternative, viz., he may cook his own food and take his wife's crops from her farm or store for this purpose (to which he has a theoretical right), but this exposes him to the sharp ridicule of the women and even of the men.[3]

[3] It is often pointed out in West African literature that women control the cooking pot, but, to my knowledge, there is no reference to women's control of the

The husband is careful in the exercise of any of these retaliatory measures since they may break up the marriage. The wise husband usually uses them as threats and rarely brings them into play.

The power of the ancestors in West Africa has often been discussed but this usually refers to the male ancestors. Such formal devices as calendrical sacrifices and shrines, characteristic of the male ancestors, are absent from the women's relationship to the female ancestors. Nevertheless, a great source of women's power in this area is derived from the female ancestors. For example, during a period of severe rainfall dangerous to the growth of their crops, women invoke their ancestresses and use fern-covered sticks as improvised symbols. With these sticks they draw lines across every path leading into Ozuitem to kill those human rain-makers who may be causing the excessive rainfall. During the Aba Riots, as well as at other crises, they gathered and sacrificed to these sticks to bring death to their enemies.

During women's council or court meetings, witnesses who make crucial statements must swear upon a pestle bound with a young palm leaf which derives its power to kill those who swear falsely from the female ancestors. As we have seen, a woman gravely wronged by a man may strike her pestle on her fireplace and call upon the ancestresses to kill the man.

In situations where a woman theoretically has few rights, a number of mechanisms are employed to circumvent this fact. While examples could be chosen from, among others, secret societies or the marriage situation, the following will serve to illustrate the point. Theoretically, a woman has no title to property. However, a woman can buy land or get it on pledge, but she needs a male proxy, either her husband or a close male relative, usually of her husband's extended family. This man retains formal title to the land, but this is

actual food supply. In addition to the Ibo, I have determined that a form of this control obtains among some northern Ibibo groups and Prof. C. Daryll Forde has communicated to me that an attenuated form functions among the Yakö, a semi-Bantu group on the Cross River.

only nominal. A woman may also frustrate her husband's control over her by buying or receiving land on pledge through a lover, so that the husband has no rights over this land. Furthermore, since the husband neither cuts nor burns the bush on this land, his theoretical right to the crops grown on it is effectually lessened.

The articles which the woman brings to her husband's compound at marriage belong to the husband. He purchases these household goods in a ceremonial transaction at only a fraction of their worth, symbolizing the fact that even these things do not belong to the woman and she comes to her husband with no possessions of her own. However, in case of divorce, a woman may take a number of things with her, along with a few head of sheep, through the force of her own personality and the strength of her relatives. Only in a few cases does the wife leave with no property.

Women have a number of means of exerting pressure upon men, when they feel that they have been wronged or when they wish their demands met. Such techniques as boycott, strike and, as we have seen, ridicule and cursing, are employed. A few examples illustrate some of these techniques.

1. The men of one sib were very angry because their wives were openly having relations with their lovers. The men, their pride hurt and afraid that the ancestors might cause their wives to be barren because of this flagrant adultery, met and passed a law to the effect that every woman in the sib should renounce her lover and present a goat to her husband as a token of repentance. At first, the older women were exempt from this penalty, but they directed so much ridicule toward the men that they too were included in the order. The women held small secret meetings and, a few mornings later, they went to a neighboring sib, leaving all but suckling children behind them. The men had to care for the crying children and were forced to carry water, bring firewood and to do the rest of the women's work. They endured it for a day and

a half and then, they went to the women and begged them to return. They paid the men of the other sib to intercede in their behalf. To the women, the men gave one goat and apologized informally and formally. The women returned.

2. Women of a kindred had repeatedly asked the men to order the proper age grade to clean an overgrown path to their farms and to repair a bridge. The men neglected to do this and the women refused to cook food for their husbands until this order was carried out.

3. The following incident illustrates the boycott technique. A woman refused to pay a fine which the women's council had imposed. All the other women refused to speak to her, to buy from, or sell to her in the market, and to give her live embers for her fire. Any woman who violated this boycott was to be severely fined. Within three weeks the errant woman paid her fine.

4. There is also the technique of "sitting upon a man." To the compound of a man who has wronged them, the women come singing songs of ridicule and remain there, sometimes staying overnight and longer, until the man capitulates.

As a final example of the power and importance of women, we refer briefly to the organized protest of Ibo women known in Nigeria as the Aba Riots of 1929.[4] In addition to the strong intra-community unity of women the Aba Riots were a striking illustration of women's ability to organize upon an inter-community basis, never achieved by men. In answer to a rumor swiftly spread through the market system that women were to be taxed, and quickly responding to a hurried call for concerted action in face of a common danger, more than 10,000 women drawn from various communities over a wide area marched upon the headquarters of the white administrators. They demanded concrete assurance that taxes would not be levied upon them as they were upon Ibo men two years before. The women carried with them the fern-

[4] For a description of these riots see Perham, M. *Native Administration in Nigeria.* London: Oxford University Press. 1937.

covered sticks, symbols of the protection of the female ancestors. Spokeswomen and leaders appeared to present the demands of these women from various communities. The revolts spread and culminated in a violent reaction against white acculturation.

Although, theoretically, this is a patrilineal society and formal controls are in the hands of men, as has been shown. the women exercise considerable power in their councils and courts, in their rôles as child-bearers and food-producers and through the religious sanction of the female ancestors. Even where women have no formal methods of gaining their ends, they can resort to such ingenious techniques as ridicule, boycott and strike. In addition to their power within the community, women have developed a mechanism for "international" solidarity, as displayed in the Aba Riots, which the men have never achieved.

SECTION OF PHYSICS AND CHEMISTRY

FEBRUARY 2 AND 3, 1940

CONFERENCE on "Physical, Physical-Chemical and Organic-Chemical Evidence Regarding Crystalline Protein Molecules."[1]

Professor Edwin J. Cohn, Department of Physical Chemistry, Harvard University, was in charge of this meeting as Conference Chairman. The program consisted of the following papers:—

"Evidence from Organic Chemistry Regarding the Composition of Protein Molecules," by Doctor H. B. Vickery, Connecticut Agricultural Experiment Station. Discussion—M. Bergmann, R. K. Cannan, H. T. Clarke, E. Brand, V. du Vigneaud, J. S. Fruton, M. Heidelberger, S. Moore, H. B. Lewis, T. L. McMeekin, R. Schoenheimer, W. H. Stein.

"Evidence from Physical Chemistry Regarding the Size and Shape of Protein Molecules. Ultracentrifugation, Diffusion, Viscosity, Dielectric Dispersion and Double Refraction of Flow," by Doctor J. L. Oncley, Harvard Medical School. Discussion—J. H. Bauer, J. T. Edsall, J. D. Ferry, R. M. Fuoss, J. G. Kirkwood, E. O. Kraemer, M. A. Lauffer, J. W. Mehl, H. Mueller, H. Neurath, J. H. Northrop, L. Onsager, E. G. Pickels, G. Scatchard, W. M. Stanley, J. W. Williams.

"Evidence from X-Rays Regarding the Structure of Protein Molecules," by Doctor B. Warren and Doctor I. Fankuchen, Massachusetts Institute of Technology. Discussion—D. Harker, M. L. Huggins, I. Langmuir, C. E. Mooney, A. L. Patterson, D. M. Wrinch, R. W. G. Wyckoff.

[1] Only molecules that appear to be chemical individuals and that have been thus far studied by several methods were considered. These included egg albumin, lactoglobulin, insulin, hemoglobin, chymotrypsin, serum albumin and edestin.

NEW MEMBERS

Elected February 5, 1940

SUSTAINING MEMBER

Joyner, A. L., M.D., Director, Immunological Research, Lederle Laboratories, Pearl River, N. Y.

ACTIVE MEMBERS

Ackermann, Alfred J., M.D., Instructor, Radiology, School of Medicine, University of Oklahoma, Oklahoma City, Okla.

Akin, Russell B., Ph.D., Research Chemist, E. I. duPont de Nemours Co., Arlington, N. J.

Asch, Solomon E., Ph.D., Assistant Professor, Psychology, Brooklyn College, Brooklyn, N. Y.

Bolling, Diana, B.S., Research Fellow, N. Y. State Psychiatric Institute and Hospital, New York, N. Y.

Brescia, Frank, Ph.D., Instructor, Chemistry, College of the City of New York, N. Y.

Collins, James A., M.B.A., Statistician, New York, N. Y.

Coren, Malvin M., M.D., Instructor, Anatomy, New York Medical College, New York, N. Y.

Evans, Mary J., Ph.D., M.D., Instructor, Bacteriology, Women's Medical College, Philadelphia, Pa.

Johnson, Rupert H., A.B., West Point graduate, Investment Broker, New York, N. Y.

Hardy, James D., Ph.D., Research Associate, Russell Sage Institute of Pathology, New York, N. Y.

Hawthorne, Margaret J., B.S., Instructor, Supervisor, School of Nursing, Presbyterian Hospital, New York, N. Y.

Landry, Herbert A., Ph.D., Research Assistant, Board of Education, New York, N. Y.

Lehman, Robert A., Ph.D., Instructor, Therapeutics, College of Medicine, New York University, N. Y.

Novalis, Nicholas Arthur, M.D., Instructor, Anatomy, New York Medical College, New York, N. Y.

Parker, Raymond C., Ph.D., Head, Virus Division, Biological Laboratories, E. R. Squibb & Sons, New Brunswick, N. J.

Pignatelli, Myrtle E. L., M.A., Clinical Psychologist, Bellevue Psychiatric Hospital, New York, N. Y.

Scarff, John Edwin, M.D., Assistant Professor, Clinical Neurological Surgery, Columbia University, New York, N. Y.

Smith, Emil L., Ph.D., Research Fellow, Physiological Chemistry, Yale University, New Haven, Conn.

Stein, William H., Ph.D., Assistant, Biochemistry, Rockefeller Institute, New York, N. Y.

Temple, William J., Ph.D., Instructor, Speech, Brooklyn College, Brooklyn, N. Y.

Thomas, Bernard O. A., D.D.S., Research Assistant, Histology, Columbia University, New York, N. Y.

Trask, James D., M.D., Associate Professor, Pediatrics, Yale University, New Haven, Conn.

Urey, Harold Clayton, Ph.D., Sc.D., Professor and Executive Officer, Chemistry, Columbia University, New York, N. Y.

Warfel, Herbert E., M.S., Assistant Professor, Zoology, University of New Hampshire, Durham, N. H.

Wetmore, Alexander, Ph.D., D.Sc., Assistant Secretary, Smithsonian Institution, in charge U. S. National Museum, Washington, D. C.

Zahl, Paul Arthur, Ph.D., Research Associate, Haskins Laboratories, Schenectady, N. Y.

ASSOCIATE MEMBERS

Ashley, Samuel E. Q., M.A., In charge, Analytical Laboratory, General Electric Co., Pittsfield, Mass.

Biberman, Lucien M., Student, Rensselaer Polytechnic Institute, Troy, N. Y.

Bowles, Edgar, Ph.D., Geologist, Geological Survey of Alabama, Tuscaloosa, Ala.

Cook, Roy Selden, Ph.D., Professor, Chemistry; Head, Science Dept., Mary Washington College, Fredericksburg, Va.

Derow, Matthew Arnold, M.D., Instructor, Bacteriology and Immunity, School of Medicine, Boston University, Boston, Mass.

Hamilton, J. Bruce, A.B., Reference Assistant, Science Dept., Boston Public Library, Boston, Mass.

Renaud, E. B., Ph.D., Professor, Anthropology, University of Denver, Denver, Colo.

Scatchard, George, Ph.D., Professor, Physical Chemistry, Massachusetts Institute of Technology, Cambridge, Mass.

Steele, N. Dudley, M.S., Chemical Engineer, Godfrey L. Cabot, Inc., Pampa, Texas.

van Diest, Alice E., M.A., Professor, Sociology and Social Work, Colorado College, Colorado Springs, Colo.

Waldo, Allen W., Ph.D., Assistant Professor, College of the Pacific, Stockton, Calif.

Waugh, Karl T., Ph.D., LL.D., Personnel Officer, Dept. Public Assistance of Pennsylvania, Harrisburg, Pa.

Welch, d'Alte A., Ph.D., Zoology Department, Johns Hopkins University, Baltimore, Md.

Winder, Claude V., Sc.D. Instructor, Physiology, University of Michigan, Ann Arbor, Mich.

Zapffe, Carl A., Sc.D., Research Associate, Battelle Memorial Institute, Columbus, Ohio.

STUDENT MEMBERS

Osterud, K. L., A.B., Graduate Assistant, Biology, New York University, New York, N. Y.

Parke, Roger I., M.A., General Science Instructor, New York School for the Deaf, New York, N. Y.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 APRIL, 1940 No. 6

SECTION OF GEOLOGY AND MINERALOGY

MARCH 4, 1940

DOCTOR EDWIN D. MCKEE, Yale University: *Permian Deposits of the Arizona-Utah Basin.* (This lecture was illustrated by lantern slides.)[1]

The Paleozoic depositional basin located in what is now northern Arizona and southern Utah was an eastward extending embayment subsidiary to the great north-south trending geosyncline of the Nevada area. For every thousand feet of sediment deposited in the main trough during this era, only a few hundred feet of material was laid down in this area to the east. During parts of the Cambrian, Devonian, Mississippian and Permian periods, seas spread from Nevada across it, and marine deposition took place in these. During parts of the Permian period extensive continental sediments also were laid down in the area. However, at most other times (Upper Cambrian, Ordovician, Silurian, Upper Mississippian and early Pennsylvanian) no sediments corresponding to those deposited in the Nevada geosyncline were accumulated in the less developed basin of Arizona and Utah.

1 This lecture was announced under the title "Middle Permian Seas of Northern Arizona."

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 2, No. 6, April, 1940.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

Paleozoic seas advancing eastward into the Arizona-Utah basin extended at no time farther than the eastern limits of these states, although some continental sediments were deposited in the area beyond. The southern limits of the basin were also clearly defined, for in central Arizona there is a granitic positive element, called Mazatzal-land, which successfully separated most of the corresponding faunas and sediments of northern and southern Arizona during the Paleozoic. On the other hand, even though some of the clastic sediments deposited in the basin were derived from the north, its boundaries in that direction are not well known because of a lack of exposures in central Utah. In this Arizona-Utah basin, by far the greatest accumulation of sediments was during the Permian period when strata 2,500 feet thick, both continental and marine, were laid down, leaving a record which today is beautifully exposed in the walls of Grand Canyon and other canyons of the plateau region.

In the classical Grand Canyon section and elsewhere throughout a wide area, the Permian formations, from bottom to top, consist, first, of two series of red beds of deltaic origin; next, of an aeolian sandstone; and, finally, of two series of cyclic deposits which are largely of marine origin. Detailed studies of all five of these major subdivisions have been undertaken with a view toward demonstrating the nature both of the vertical and of the lateral changes within and between them, of emphasizing the principal contrasts resulting from the different types of environment which they represent, and of determining criteria for the recognition of each. These studies have consisted chiefly of a detailed tracing of beds by means of carefully measured sections and of statistical analyses of cross-lamination patterns in various units. They have been supplemented in certain critical horizons and localities by mechanical and chemical analyses, by thin-section and polished surface examination and by paleontological studies.

In the two red-bed formations which make up the lower portion of the Permian section, it has been possible to demon-

strate through detailed studies of the cross-lamination many features regarding the directions of movement among the sediments and concerning the general character of deposition on the delta surfaces. Some measure of the rapidity of delta growth has been ascertained by detailed examination of the cyclic character of the sediments; data on the nature of lateral changes toward marine facies, through the tracing of beds. On these latter problems much remains to be done.

The aeolian sandstone in the middle of the section provides an excellent example of a desert dune deposit, and so makes an especially interesting comparison with formations representing other sand environments. The general character of the dunes and the regional direction of their movement have been determined and data have been obtained which help to differentiate this type of deposit from analogous ones.

The two uppermost formations are different in many details, but have in common the fact that both are composed of varied types of sediments which were deposited during the stages of an advancing and retreating sea. Furthermore, it is possible to subdivide each of these formations into natural lateral units or facies based on observable changes in fauna and lithology. Special attention has been given the distribution—in relation to facies and horizons—of gypsum deposits and of both bedded and nodular chert, and it has been found that most evidence favors a primary origin in each case. The distribution of the magnesium content in the limestones also suggests a relationship with the environment of deposition.

The Permian basin deposits of northern Arizona offer exceptional opportunities for detailed work, both because of the unique character of the exposures and because of the contrasts in the types that are represented. When these formations have been studied in sufficient detail, they should serve to demonstrate many of the principles of stratigraphy and sedimentation which are less completely available for study in other regions.

SECTION OF BIOLOGY

March 11, 1940

Professor E. R. Dunn, Haverford College: *Some Aspects of Herpetology in Lower Central America.* (This lecture was illustrated by lantern slides.)

The herpetological fauna of Nicaragua, Costa Rica, and Panama is very rich, consisting as it does of 7 orders, 26 families, 124 genera, and 375 distinguishable forms. Almost the entire fauna can be found in the area between the Canal Zone and the Costa Rica-Nicaragua border. Nicaragua has only 4 genera and 24 species which do not occur in Costa Rica, and Darien has only 6 genera and 28 species which are not found in the Canal Zone to the west. The important barrier is the mountain mass on the Panama-Costa Rica border, and therefore Costa Rica and western Panama together have a fauna richer in families and genera than the areas immediately to the north and south. There are no endemic families in the central area and only one endemic genus, but no less than 157 species are endemic. I cannot as yet account for this richness in species, nor for the amount of specific endemism.

The fauna of the Pacific side savanna country has affinities with that of the Caribbean coast of South America, which likewise has a marked dry season. The fauna of the Atlantic side rain-forest has affinities with that of the Pacific coast rain-forest of Colombia. There is thus a faunistic crossing over in the Canal Zone and in Darien.

The fauna of the adjacent islands is a depauperate mainland one. The fauna of the Greater Antilles is likewise depauperate as compared to that of the mainland. Two orders, 10 families, and 100 genera are not represented there. This marked decrease is thought to be evidence against former land connection. About half the Greater Antillean fauna may be derived from that of the opposite mainland of northern Central America and Mexico. About a quarter of the

fauna is derivable from forms which now reach their northern limit in Panama or in Costa Rica. The remainder cannot be derived from any forms now in North America. This is thought to indicate a considerable southward movement of the mainland fauna.

In Lower Central America the climatic and botanical areas are wet tropics (rain-forest), wet and dry tropics (monsoon forest, savanna), and wet temperate (fog-forest). Ecologically, there are two terrains in the tropical forested regions. One includes the baseleveled regions on the flood plains of the larger rivers; the other is the hilly or mountainous country not yet reduced to base level. In the former, there is some open country along the streams and much still water. In the latter, there is little open country and the processes of erosion in the humid tropics produce very steep slopes and gully formation down to bed rock. There is, therefore, very little still water and the ravines are not safe habitats for aquatic animals. In these areas there are no water snakes and frogs have adopted many methods of avoiding water in the early stages of their life histories. This ecological analysis is believed to be true for the forested tropics in general. There seems no good evidence for an amphibian or reptile fauna restricted to the higher levels of the trees.

Study of some 9,500 snakes from four Panamanian areas, collected at random for census purposes, indicates that the dominate snake species differ from area to area, although the areas may have similar conditions and a similar list of snake species. In each area a few forms are abundant and the rest are rare. Roughly, 10% of the species make up half the population; while half of the species make up 5% of the population. This situation has been found to characterize all snake faunas examined (in other tropical areas, San Diego Co., Cal., and Eastern United States). A species may be generally abundant without being dominant anywhere; it may be dominant in one area and extremely rare everywhere else; but there is some correlation between dominance and wideness

of range. Data over a series of years show fluctuations as well as steady increases in some species, and steady decreases in others.

The data from these snakes indicate that there is no correlation between number of species in a group and abundance of individuals of that group in nature.

SECTION OF PSYCHOLOGY

March 18, 1940

Doctor Kurt Goldstein, Montefiore Hospital, New York, N. Y.: *Significance of Speech Disturbances for Normal Psychology*. (This lecture was illustrated by motion pictures.)

The following explanations will take into consideration only such pathological findings which are related to two special problems in the psychology of speech:—(1) the problem of meaning of words; (2) the significance of "speech automatisms."

There are psychologists who deny that there is such a phenomenon as the meaning of words. To them words are nothing but associations between sounds or movements and objects or situations. Speech, then, would be a kind of conditioned response.

To prove that such an assumption is incorrect, there are no more suitable data than the behavior of aphasic patients whose words have lost meaning. Let us begin with a simple example.[1] There lies on the table a heap of colored woolen skeins of many colors and different shades. A normal person, asked to pick out all reds or all greens, picks all the various shades belonging to red or green. The patient, however, picks up only one or a few very similar skeins of the named color. It appears that the word, "green," fits for him only one special object or very few of close similarity. When the patient is asked to name the various colors, he calls only a very few skeins "green" or "red." He is not able to name at all the other shades of the same hue, or he calls them by special names which fit the special appearance of the individual color (*i.e.*, "light green," "moss green," "this is the color of the leaves," etc.). Faced with all the color shades

[1] K. Goldstein The Problem of the Meaning of Words Based Upon Observation of Aphasic Patients. *Jour. of Psychol*, 2, 1936.

which we call green, he denies that one can call them all green. The patient shows the same behavior as to other objects. Asked for the name of an object (knife), a woman-patient called it either an apple-parer, or a bread-knife, or a pencil-sharpener or a potato-pealer, according to the various situations in which the same knife was presented to her. A knife together with a fork, she called "knife-and-fork," and she objected to speaking simply of a knife in various situations, as, for example, in connection with an apple or a pencil. This incapacity to use the same word in different situations manifests itself particularly when the patient has to deal with words which have several meanings and a different meaning in different situations. A good example, for instance, would be the many meanings of the word "bar." The patient might be able to utter such a word in one of its various connections, but immediately thereafter, he would be unable to use or understand it in one of the other senses. We might say that the patient seems to be able to use words only if they were associated with a definite individual object or situation. He has only individual words, belonging to an object like other properties, *e.g.*, color, form, etc., but he cannot use the word if it merely represents the abstract class or category under which the object in question may be classified. He cannot use words as symbols. The words have lost meaning in themselves.

This speech defect goes along with an impairment of the conceptual approach toward the world in general.[2] The patient uses objects only as means for concrete activities, not as examples or representations of a class. This behavior of the patients becomes especially clear in their reaction to some special tests,[3] which the author has contrived for investigations of the impairment of the "categorical" or "abstract" attitude (Color Sorting Test, Object Sorting Test, Block

[2] K. Goldstein: The Significance of the Frontal Lobes for Mental Performances. Jour. of Neurol. and Psychopath., XVII. 1936.

[3] K. Goldstein and M. Scheerer: Abstract and Concrete Behavior. An Experimental Study with Special Mental Test. (In print.)

Design Test, etc.). The loss of meaning of words and the impairment of the abstract attitude and reduction to a more concrete level of behavior is, in the speaker's opinion, based on one and the same *change of the personality as a whole.*

The significance of words becomes obvious when one considers the peculiarities which speech shows under this condition. The whole language is changed from an active, spontaneous *productive means* for expressing ideas, feelings, etc., to a passive, more or less compulsive, stereotyped, and unproductive *reaction.* The amount of speech is generally reduced, especially spontaneous speech. The patients have the greatest difficulty in beginning to speak. Speaking has to be elicited through outside stimuli. When stimulated to speak, the patients have difficulty in stopping and use many stereotyped utterances, etc.

One especially interesting consequence of the modified character of speech is the fact that the patients have the greatest difficulty in *speaking so-called senseless sentences,* or even to repeat them on request. For example, such as are in contrast to a given situation or to their knowledge. One of my patients was unable to repeat the sentence: "It is raining today" on a day of sunshine; or to repeat "The snow is black," or "2×2 are 5." Such patients cannot understand how to say so-called senseless things because it is possible to understand senseless sentences only if one can abstract from the given situation or from experienced facts.

The great change in the patient's world involved in this loss of the meaning of words, as such, would make the patient totally helpless if he did not retain, from the time before the onset of the disease, many automatisms, with the help of which he can come to terms with his environment even though he has no real consciousness of their action, which would naturally require abstract mental power.

Analysis of these automatic speech reactions reveals to how great a degree the speech of adult human being is independent of his *personality* core, and discloses the associa-

tive character of much of his speech. One especially interesting example are the vocabularies which we acquire in learning a foreign language. So long as we have no real understanding of the foreign language we acquire the words only in their superficial connection with the words of our own language. We know that these words belong to a definite situation and are able to use them correctly without having a real understanding of their meaning. However, this is the case only in these situations. We make many a mistake as the consequence of the fact that we utter them in situations where they do not fit, because we have no real insight as to their meaning. The situation changes when we have acquired a real conception of the foreign language, so that we understand the fundamental meaning behind its words. Then the words achieve an absolutely different character. They become representatives of the categorical approach to the surrounding world, and then only, one is justified to speak of mastering the language.

Pathology teaches us further how strongly these automatic reactions depend upon the categorical attitude, both for their *acquisition,* as well as their *preservation* in memory.

The speaker pointed to the practical and theoretical consequences of the pathological findings for the customary theory of association and conditioning, and the implications of his results as affecting the theory of learning and the origin of speech in the history of man. He comes to the conclusion that human language and meaning cannot be separated. Human language is never a merely external association. Language involves a special attitude toward the world,—that attitude in which the individual is detached from a given condition, *i.e.,* the attitude characteristic of the human being. *When first there was a human being, there was meaningful speech.* This does not mean that normal language is always meaningful speech and that normal performances are carried out exclusively on the basis of the abstract attitude. In ordinary life, the concrete behavior plays a very great part and most of our performances are carried out in a concrete way.

But the mental set-up which these performances require as their conditional background is the abstract attitude. Otherwise, we would simply be robots or behave like the patients above described. Thus, the abstract attitude is essential to the human being. Its importance can be nowhere grasped so clearly as in the change of the behavior in patients under observation; in their lack of activity, creativeness, freedom, social adaptation, and in the changes of their language.

The speaker finished with some remarks concerning the relationship between the findings in the patients and observations on so-called primitive people. He pointed to the similarities with respect to behavior in general and speech in particular, but rejects the assumption that primitive people represent a defective condition like that of the patients above described, or a less developed kind of human beings, or that their capacities are of a degree inferior to those of "civilized" people. That "primitive" people behave concretely is no justification for assuming that they are not able to behave in other ways under varied conditions. The investigations, with our tests, in normal persons belonging to our culture are very instructive in this respect. Not seldom do we observe that very intelligent persons show a preference for concrete responses in these tests. However, these subjects are able to shift from the concrete to the abstract procedure if asked to do so, or if the problem so demands it. This is the decisive difference between the normal person and the patient, since the patient cannot shift to the abstract approach, which he has lost, and it is also my opinion that this is the difference between primitive people and our patients. The reason for the concreteness evidenced in both is totally different in the two cases. The patients are forced to react concretely because of a defect in their capacities. Primitive people prefer the concrete behavior because of the special form of their culture and because it usually does not demand the activation of abstract behavior. Why this latter is the case, is a question which involves another problem very well worth discussion, and which cannot be dealt with at the present time.

NEW MEMBERS

Elected March 4, 1940

SUSTAINING MEMBERS

Eising, Eugene H., M.D., Surgeon, New York City.

Grace, Arthur William, D.P.H., D.T.M. and H. (England), Professor, Dermatology and Syphilology, Long Island College of Medicine, Brooklyn, N. Y.

ACTIVE MEMBERS

Adamic, Louis, Writer, Milford, New Jersey.

Altman, Sylvia, M.A., Psychologist, New York, N. Y.

Anderson, Rubert S., Ph.D., Biophysicist, Memorial Hospital, New York, N. Y.

Atz, James Wade, A.B., Ichthyologist, New York Aquarium, New York, N. Y.

Babor, Joseph A., Ph.D., Division of Physical Chemistry, College of the City of New York, New York, N. Y.

Brown, Franklin B., M.A., Instructor, Chemistry, College of the City of New York, New York, N. Y.

Cerecedo, L. R., Ch.E., Ph.D., Professor, Biochemistry, Fordham University, New York, N. Y.

Chase, Aurin M., Ph.D., Research Associate, Physiology, Biology Laboratories, Princeton University, Princeton, N. J.

Fisher, George J., M.D., Deputy Chief Scout Executive, Boy Scouts of America, New York, N. Y.

Gorin, Manuel H., Ph.D., Investigator, Chemistry and Physics, New York, N. Y.

Hoagland, Charles Lee, M.D., Associate, Biological Chemistry, Rockefeller Institute; Physician-in-Residence, Rockefeller Hospital, New York, N. Y.

Hanssen, Eilif Carl, M.D., Assistant Visiting Surgeon, N. Y. Post Graduate and Bellevue Hospital, Associate Visiting Surgeon, Rikers Island Hospital, New York, N. Y.

Kabat, Elvin A., Ph.D., Instructor, Pathology, Cornell University Medical College, New York, N. Y.

Ketchum, Bostwick Hawley, Ph.D., Instructor, Biology, Long Island University, Brooklyn, N. Y.

Kondritzer, Albert A., Ph.D., Research Associate, Biochemistry, N. Y. State Psychiatric Institute, New York, N. Y.

Lester, James George, Ph.D., Professor, Geology, Emory University, Georgia.

Levin, S. Benedict, E.M., Instructor, Geology, Hunter College, New York, N. Y.

Light, Amos Ellis, Assistant Biochemist, Burroughs Wellcome & Co., U. S. A. Experimental Research Laboratories, Tuckahoe, N. Y.

Milch, Henry, M.D., Orthopedic Surgeon, Riverside Hospital; Hospital for Joint Diseases; Instructor, Anatomy, Columbia University, New York, N. Y.

Moore, Robert Jerome, Ph.D., Development Manager, Varnish Resin Division, Bakelite Corp., Bloomfield, N. J.

Nachod, Frederick C., D.Sc. (Utrecht), Research Fellow, Columbia University; Instructor, Chemistry, College of the City of New York, N. Y.

Naumburg, Robert E., S.B., Mechanical Engineer, Jonas & Naumburg Corp.; President, Visagraph Institute for Blind, New York, N. Y.

Nigrelli, Ross F., Ph.D., Pathologist, New York Aquarium, New York, N. Y.

Paul, Allard Anthony, Ph.D., Instructor, Animal Parasitology, College of the City of New York, N. Y.

Reiner, Laszlo, M.D., Ph.D., Director of Research, Burroughs Wellcome & Co., U. S. A., Experimental Research Laboratories, Tuckahoe, N. Y.

Smith, Clifton A. H., D.M.D., New York, N. Y.

Stern, Kurt Guenter, Ph.D., Research Assistant Professor, Physiological Chemistry, Yale University School of Medicine, New Haven, Conn.

Stone, Irving R., M.A., Instructor, Psychology, Long Island University, Brooklyn, N. Y.

Tobin, Elise, Ph.D., Assistant Professor, Chemistry, Brooklyn College, Brooklyn, N. Y.

Tuma, Vladimir, C.E., Ph.D., Laboratory Director, S. H. Kress & Co., New York, N. Y.

Turner, Francis M., M.Sc., Vice-President, Reinhold Publishing Corp., New York, N. Y.

Vickery, Hubert Bradford, Ph.D., Biochemist in charge, Biochemical Laboratory, Carnegie Institution, Connecticut Experiment Station, New Haven, Conn.

Winterble, Margaret R. M., M.A., Research Assistant, Educational Measurements, Board of Education, New York, N. Y.

ASSOCIATE MEMBERS

Arnold, Orlan M., Ph.D., Instructor, Chemistry and Chemical Engineering, Rensselaer Polytechnic Institute, Troy, N. Y.

Barclay, Gordon Laniar, B.S., Assistant Professor, Psychology and Education; Acting Director, School of Education, Russell Sage College, Troy, N. Y.

Barkan, Georg, M.D., Instructor, Pharmacology, School of Medicine, Boston University, Boston, Mass.

Bausor, Sidney C., Ph.D., Instructor, Biology, Lehigh University, Bethlehem, Pa.

Boyd, William C., Ph.D., Associate Professor, Biochemistry, School of Medicine, Boston University, Boston, Mass.

Caster, Kenneth E., Ph.D., Curator, University of Cincinnati Museum, Cincinnati, Ohio.

Cole, Robert H., A.M., Instructor, Physics, Harvard University, Cambridge, Mass.

Crickmay, Geoffrey W., Ph.D., Associate Professor, Geology, University of Georgia, Athens, Ga.

Ellis, George R., D.D.S., Dental Consultant, Providence Hospital, Washington, D. C.

Elvehjem, Conrad A., Ph.D., Professor, Biochemistry, University of Wisconsin, Madison, Wis.

Farish, Linn M., B.A., District Geologist, Magnolia Petroleum Company, Youngstown, Ohio.

Fellers, Carl Raymond, Ph.D., Research Professor of Food Technology; Research Chemist, Massachusetts State College, Amherst, Mass.

Ferry, John Douglass, Ph.D., Junior Fellow, Physical Chemistry, Society of Fellows, Harvard Medical School, Harvard University, Boston, Mass.

Laurence, Robert A., M.A., Associate Geologist, Tennessee Valley Authority, Knoxville, Tenn.

MacEwen, Ewen Murchison, M.D., Dean, College of Medicine; Head, Department of Anatomy, State University of Iowa, Iowa City, Iowa.

MacNider, William deB., M.D., Dean, School of Medicine, University of North Carolina, Chapel Hill, N. C.

Stadherr, Nicholas George, M.S., Mechanical Supervisor, Injection Molding Department, E. I. duPont de Nemours & Co., Leominster, Mass.

Stultz, Walter Alva, Ph.D., Assistant Professor, Anatomy, College of Medicine, University of Vermont, Burlington, Vt.

Thode, Henry George, Ph.D., Assistant Professor, Chemistry, McMaster University, Hamilton, Ontario, Canada.

Tower, Olin Freeman, Ph.D., Professor, Chemistry, Western Reserve University, Cleveland, Ohio.

True, Nathan F., Sc.D., Chief Chemist, Mead, Johnson & Co., Evansville, Ind.

Tulane, Victor J., Ph.D., Assistant Professor, Chemistry, Howard University, Washington, D. C.

Upson, Walter L., E.E., M.S., M.E.E., Director of Research, The Torrington Mfg. Co., Litchfield, Conn.

Vetter, J. M., A.B., Superintendent Land & Geological Department, Pan American Production Co., Houston, Texas.

STUDENT MEMBERS

Block, Paul, Jr., M.A., Graduate Research Student, Chemistry, Columbia University, New York, N. Y.

Coles, James Stacy, A.M., Graduate Research Student, Columbia University; Tutor, College of the City of New York, N. Y.

Jones, Marshall B., M.A., Assistant, Psychology, Cornell University Medical College; Psychologist, New York Hospital, New York, N. Y.

Price, Fraser P., Graduate Student, Chemistry, Columbia University, New York, N. Y.

Schlobohm, Olga, M.A., School Psychologist, New York, N. Y.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 MAY, 1940 No. 7

SECTION OF GEOLOGY AND MINERALOGY

APRIL 1, 1940

DOCTOR WALTER H. BUCHER, University of Cincinnati, Cincinnati, Ohio: *The Geology of the Cody Region.*

The geology of the Cody region comprises three major elements: (a) the broad, asymmetrical uplifts that surround the Bighorn Basin on three sides; (b) the scattered thrust masses of which Heart Mountain is the best known; and (c) the products of vulcanism of the Absaroka Mountains and the Yellowstone Park region. The chief purpose of the lecture was to bring out clearly the incongruous character of the thrust masses in the general regional setting.

1. The Beartooth Mountains illustrate the structural character of the uplifts. In the Beartooth Mountains, the Pre-Cambrian basement lies exposed for a distance of some 80 miles along the northwest-trending axis of the uplift, apparently forming a single unit. But throughout its whole width, from its northeastern foothills to the region around Gardiner in the southwest, the northern half of the uplift (including the Snowy Mountains) is dominated by movement from the northwest to the southeast, all secondary structures, such as folds and thrust-planes being overturned toward the southwest.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 2, No. 7, May, 1940.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

The southeastern half of the Beartooth Mountains, on the other hand, from the Stillwater River to the plunging southern end of the uplift at Clark Fork Canyon, has been moved northeastward, the long dip-slope of the range sloping gently toward the southwest, while the front lies overthrust toward the northeast on to the Paleocene rocks of the foreland, individual units having been pushed forward differentially along tear faults.

Southeast of the Beartooth Mountains, in the direction toward Cody, the Paleozoic rocks emerge again from beneath the Tertiary cover to form two large anticlines, namely Pat O'Hara and Rattlesnake Mountains. The structure of both indicates movement in the opposite sense from that of the adjoining part of the Beartooth Mountains, the Rattlesnake Mountain fold especially being steeply upturned toward the southwest.

The same reversal of asymmetry along the axis of an uplift is exhibited on an even larger scale by the Bighorn Mountains. Here a northern and a southern portion were moved southwestward, while the central part was overthrust eastward between the latitudes of Sheridan and Buffalo, Wyoming.

In these, as in such minor uplifts as the Pryor and Owl Creek Mountains, the direction of overthrusting was clearly determined by local conditions (the Owl Creek Mountains are overthrust southward toward the deep Wind River basin). The asymmetries of the minor folds which line the borders of the Bighorn Basin, agree with those of the adjoining parts of the major uplifts. The whole structure is utterly different from that which characterizes the foreland of every major thrust belt known to the writer. It is the more surprising, therefore, to find in the Cody region isolated thrust masses which have been interpreted as the last erosion remnants of a huge thrust sheet which, if it actually existed, must have been one of the greatest of the continent.

2. The thrust masses consist of thick-bedded Paleozoic

rocks (Bighorn dolomite, Devonian limestones and shales, and Madison limestone) which have come to lie on Cretaceous and Tertiary rocks. Six such masses have been known since L. C. Dake and D. F. Hewett first recognized their nature. They seem to lie in two belts—an outer semicircular one, including Heart Mountain, McCulloch Peaks, and the small mass at the north end of Carter Mountain; and an inner one, comprising the mass near Bald Ridge, Logan Mountain, and Sheep Mountain, the largest of all. (See map in W. H. Bucher, W. T. Thom, Jr., and R. T. Chamberlin, "Geologic Problems of the Beartooth-Bighorn Region," *Bull. G. S. A.,* 1934, 45, opp. p. 167).

To the orthodox geologist it is a foregone conclusion that such widely scattered limestone masses are erosion remnants of a once continuous thrust-sheet. The speaker thought so himself at first. The detailed description of the observations that have caused him to reject that view will be given in the report that is in preparation. A few will be mentioned briefly:

a. The dimensions of the supposed original thrust sheet must have been at least of the order of 40 by 40 miles. Since the so-called erosion remnants consist only of Paleozoic limestones, the thrust sheet must have been formed by "peeling-off" the beds above the Cambrian. Such a process on such a scale is entirely out of harmony with the regional structural pattern.

b. If the thrust sheet had been formed in that manner, minor thrust faulting and imbrication should be a conspicuous feature of the internal structure of the preserved portions. None could be found. Instead, "normal" faulting and corresponding jointing dominate their structure.

c. The outermost, completely shattered fragments have escaped destruction by weathering, while over 99 per cent of the supposed thrust sheet, especially the portions that lay nearer the origin, have vanished completely.

d. The amount of weathering and erosion that had to be

accomplished to remove almost all of the supposed thrust sheet is entirely out of proportion to the available time.

e. The minor structures, which are exposed beneath the surface on which the thrust masses rest, are arranged with reference to a center (near Trout Peak), which serves equally well as the center of the semicircle into which the thrust masses may be grouped.

3. The time at which the thrust blocks reached their present position constitutes one of the most important questions connected with this problem. Here, also, our views are undergoing a change, but time did not permit a discussion of the stratigraphic and structural detail necessary for a satisfactory answer. One thing is obvious, namely, the thrusting took place before the beginning of the violent volcanic activity, which produced the extraordinarily coarse breccias that form the lower cliffs along South and North Forks of Shoshone River.

If the thrust masses are not erosion remnants of a once continuous thrust sheet, they are the product of a form of deformation, the nature of which is not understood as yet. Two regions are known to the speaker in which strong deformation was produced radially outward from a center—the belt of faulting that surrounds the Bearpaw Mountains in Montana, and the faulting and thrusting in and around the Ries basin in southern Germany.

SECTION OF BIOLOGY

April 8, 1940

Doctor Grace E. Pickford, Bingham Oceanographic Laboratory, Yale University and Albertus Magnus College, New Haven, Conn.: *The Vampyromorpha,* Living-Fossil Cephalopoda. (This lecture was illustrated by lantern slides.)

The name *Vampyroteuthis infernalis* was given by Chun to three specimens of a little black cephalopod which were captured by the "Valdivia" Expedition in the great depths of the Atlantic Ocean. Water color drawings were made by Rübsamen, the artist of the expedition. One of these, a side view, was published by Chun in 1903 in his book "Aus den Tiefen des Weltmeeres." It showed a black octopus-like creature with extensive development of the web and complete dorsal union of head and mantle. Near the apex of the body were *two* pairs of paddle-shaped fins. The arms bore pairs of cirri alternating with the suckers. Other cirrate-armed, fin-bearing Octopoda were already known and the Vampyroteuthidæ were unhesitatingly placed with them by earlier investigators of the group.

In later editions of his popular book and in the final description of the species as prepared by Thiele in 1915, after Chun's death, one of the two pairs of fins was deleted from Rübsamen's figure. The originals are still in the Zoological Museum of the University of Berlin, and one can see where the posterior fin was erased from the drawing.

Although the "little black octopus" is always rare, and usually badly rubbed and in poor condition when captured, there have been few deep sea expeditions which have not brought back at least one or two specimens. The largest numbers have been taken by the "Dana" Expeditions and, in all, there are now about seventy-five known specimens available for study, although a half of these—mostly those

captured by the 1928–30 "Dana" Expedition—have not been reported on as yet. Those captured by the 1920–21 "Dana" Expedition were investigated by Joubin (Final report: 1937).

Joubin was the first to notice that both two- and four-finned specimens existed and, as he rightly suggested, it turned out subsequently that Rübsamen's original figure was correct and that the type of *Vampyroteuthis infernalis* is actually provided with two pairs of fins. Various other supposedly significant taxonomic differences were recognized and, in 1938, there were as many as eleven species on record. Robson, in his "Monograph of the Recent Cephalopoda, Part II" (1932) (to which the student is referred for a comprehensive bibliography of the group), admitted eight genera, of which seven were monotypic.

As a matter of fact, in spite of Robson's attempt at clarification, the taxonomy of the vampyromorphs has been in a state of complete confusion. One author's criterion of description differed so greatly from another's that a comparison of species was often impracticable. Fortunately, it has recently been possible to make a re-examination of all named specimens. The results are as satisfactory as they were unexpected since it is now almost certain that only a single species exists. Many characters to which importance had been attached proved to be the result of poor preservation or incomplete observation, while others were directly related to the age of the animal. It can be shown that very young larvæ have a single pair of posterior or larval fins; that later these are resorbed and replaced by a more anterior pair of adult fins; but that larvæ of intermediate size may have both larval and adult fins, hence the four-finned specimens represented by the type itself. Only one species, *Lætmoteuthis lugubris* (Berry), was found to be incorrectly placed among the Vampyromorpha. This animal is actually a very damaged Eledoninid.

As the number of described specimens increased, it became evident that the Vampyroteuthidæ were not as closely related

to the other finned, cirrate-armed Octopoda as had been originally supposed. Several changes of classification were proposed but the most recent and most far-reaching was that of Robson, who raised the two groups to the rank of separate suborders of the Octopoda, the Vampyromorpha and Cirromorpha respectively. Although they show some superficial resemblances, Robson found that the vampyromorphs differed from the cirromorphs in several important respects. Among other things the vampyromorphs have a pair of retractile tentacles or filaments which can be completely withdrawn into pockets in the dorso-lateral sectors of the web. These filaments were supposed to be absent in the type of *Vampyroteuthis infernalis,* but as they were generally found to be present in other members of the group they came to be regarded as characteristic of it. As a matter of fact, this supposition was fully justified since a recent examination of the types has shown that they also are provided with retractile filaments (Pickford, 1939 *b*).

The internal anatomy of the Cirromorpha became generally rather well known through the work of several investigators, notably that of Meyer and, later, of Ebersbach. Although highly specialized, and in some ways rather primitive, there does not seem to be any reason to doubt that these forms are correctly placed with the Octopoda. On the other hand, the internal anatomy of the Vampyromorpha remained virtually unknown, except for a few scattered observations by Joubin and Robson's more careful study of the badly mutilated type of "*Melanoteuthis beebei.*" Yet such little information as was accumulated suggested that major problems of cephalopod classification were at stake. In particular, what was the nature of the retractile filaments? Both Robson and Joubin suspected that they represented modified arms, yet neither quite dared to insist that the Vampyroteuthidæ were other than rather primitive Octopoda.

Some salient features in the internal anatomy of an immature female belonging to the collections of the Bingham

Oceanographic Laboratory will now be described. The author has been working on this problem for about three years and preliminary reports have been presented at meetings both in this country and in England (Pickford, 1936, 1938). However, new facts come to light as the investigation proceeds, and the account which will now be given includes certain important and hitherto unreported observations.

The body of the animal, like that of so many deep sea forms, has a gelatinous consistency. Histological investigations show that the skin is covered by a thin pavement epithelium and that the underlying tissues are well supplied with branching and anastomosing blood vessels. Since the gills are rather small for the size of the animal it seems probable that their function may be supplemented by cutaneous respiration.

The general color of the animal is black, except for the oral face of the arms and web, which is of a reddish brown. Suckers, tips of cirri, funnel and mantle interior, etc., are tinged with crimson. Microscopic study shows that whereas the crimson pigment is situated in the epithelial cells of the epidermis, the black and brown pigment granules are found in chromatophores. The chromatophores differ greatly from the highly specialized pigmentary effector organs of other dibranchiate cephalopods. No muscles are attached to the cells and the pigment granules are spread out in a single layer or strung along thin protoplasmic strands. There is no evidence as to whether color change is possible but, if it takes place, it could only be slowly through the migration of granules. Luminous organs of three types are present in all well preserved specimens but, since their appearance is well known, they need not be further described at the present time.

The subcutaneous connective tissues contain branching mesenchyme cells, leucocytes, smooth muscle cells, spirally twisted fiber cells, whose fibrous portion gives the staining reactions of collagen, and, most remarkable of all, large, swollen, ovoid cells which seem to float freely in the matrix

and appear to be chiefly responsible for the gelatinous nature of this tissue.

The body musculature is not strongly developed but can, to a great extent, be compared with the musculature of other dibranchiates. Many of the muscles, although not powerful, are evidently capable of rapid "voluntary" contraction, since they are composed of transversely striated fibers. This is true of the mantle muscle, of the funnel retractor muscles, and of the fin muscles. It is doubtful whether typical cross-striated muscle occurs elsewhere among the Cephalopoda, but in the vampyromorphs it is characteristic, since, in suitable preparations, both Hensen's line and Krause's membrane may be recognized.

The internal skeleton is very weakly developed, but one can recognize a distinct U-shaped nuchal cartilage and a delicate capsule enclosing the brain and giving support by projecting trabeculæ to muscles of the head and eyes. The histology of the cartilage has yet to be investigated, but, under high power of the binocular, it appears parenchymatous.

The gladius, on the other hand, is remarkably well developed and greatly resembles that of certain Mesozoic teuthoideans from the lithographic shales. Naef, in his book "Die fossilen Tintenfische" and elsewhere (1921, 1922), has given an interpretation of the structure of these shells and his terminology must be applied to elucidate that of the vampyromorph gladius (See diagram in Pickford, 1939 *a*).

The anterior part of the shell sac is fully exposed when the skin is removed from the dorsal surface of the body. It extends forwards to the back of the neck and is about a third the width of the body in this region. Both shell and shell sac are almost completely transparent and one looks through them, as through a window, into the interior of the body. Posteriorly, the shell sac is overlain by a thin sheet of interlacing muscles which unite the two fin bases. When these have been cut one can trace the underlying shell sac back to

the apex of the body where it bends ventrally, covering the peculiar apical organ, which will be described later.

Upon removal from the shell sac, the gladius is found to be a delicate, semi-transparent, horny structure. It is completely uncalcified. Posteriorly, there is a cup-shaped, unchambered conus which widens out on either side to form a broad conus-vane. The conus-vane is, in turn, confluent with the equally broad lateral plate and the line of junction between the two, the outer asymptotic line of Naef, is considerably reinforced by a thickened ridge on the under surface of the shell. A broad, anteriorly blunt pro-ostracum projects forwards. This part of the shell is excessively thin and very easily damaged.

The fins rest directly on the shell sac whose wall is thickened to form what appear to be cartilaginous supports. In other living dibranchiates, the fin cartilages, although probably derived developmentally from the shell sac, are completely separated from it in the adult animal. The condition which appears to exist in the Vampyromorpha was postulated by Naef as that which must have prevailed among the ancestral Prototeuthoidea. Brunnmüller (1929) has described the histology of what she believed to be the fin cartilage of *Vampyroteuthis infernalis,* but a re-investigation has shown that the part which she removed for sectioning was actually the rudiment of the anterior fin of a young larva with well developed, posterior fins.

One of the characters which led the early investigators to place the Vampyromorpha with the Octopoda was the dorsal continuity of head and mantle. Dissection reveals that this is not a secondary fusion of the dorsal mantle margin with the back of the head, as in the Octopoda, or, among the Decapoda, in *Sepiola.* There is no dorsal mantle communication as there is in these forms where one can pass a probe dorsally above the level of the gills from one side of the mantle chamber to the other. In some of the Cirromorpha there is evidence that such a communication has become al-

most completely obliterated, but in the vampyromorphs the anatomical relationships of shell sac and musculature preclude the possibility of such an interpretation. The anterior margin of the shell sac is directly united with the underlying nuchal cartilage by a sheet of slender muscle fibers which pass downwards and slightly backwards from its under surface. The nuchal cartilage is, in turn, united with the cephalic cartilage by the dorsal retractor muscles of the head. This condition appeared at first to be primitive and was so reported in earlier abstracts. However, recent re-examination of a very young larva captured by the "Michael Sars" Expedition and now in the Bergens Museum has necessitated a re-consideration of the problem. In this specimen, the youngest ever investigated and perhaps only recently hatched from the egg, there is as yet no development of subcutaneous gelatinous tissue. The anterior margin of the shell sac projects forwards over the back of the head and, although there is no sliding groove or locking apparatus, the resemblance to a decapod is unmistakable. The adult vampyromorph condition may thus be interpreted as a degeneration not from the Octopoda but from a decapodan-like ancestor. This is not surprising since Naef has postulated that the decapod arrangement must have prevailed among the ancestral Prototeuthoidea.

The opening into the mantle chamber extends laterally on each side to the level of the eyes. The funnel is largely sunk in the gelatinous tissues of the head. It is provided with a triangular valve and a pair of swollen funnel glands. In some vampyromorphs there was supposed to be a rudimentary locking apparatus by which the mantle margin could be held to the funnel base, as in the Decapoda. Although there is no definite structure supported by cartilage, it appears that a suction cup can be formed by the powerful funnel retractor muscles. These muscles are cylindrical, the interior core of the cylinder being filled with gelatinous connective tissue. At the base of the funnel muscle, fibers from the outer side of

the cylinder bend at right angles and continue laterally outwards into the collaris muscle, while fibers from the inner side of the cylinder enter the funnel base. There is thus created, at the junction of the funnel retractor muscle with the funnel base, a cup-shaped depression overlying the soft interior core of the muscle column. This depression can be very conspicuous when the muscle is strongly contracted and it seems probable that it must have functioned as an adhesive cup during life. There is no corresponding knob or prominence on the opposing surface of the mantle.

On looking into the mantle chamber, one finds that there is no median pallial septum nor median pallial adductor muscle. Lateral pallial adductors are also absent. This constitutes another of the many differences between vampyromorphs and the Octopoda.

The vampyromorph gills have never been properly described. Robson (1932), on the basis of a drawing by Joubin, surmised that they might be quite free as in *Nautilus*. This is not the case. Each gill is suspended by a branchial septum in which lies an apparently perfectly typical branchial gland. The detailed structure of the gill is not that of an octopod. The primary lamellae, although relatively few in number as in octopods, are built on the decapodan plan. Very young animals may have as few as three or four primary lamellæ in each demibranch. The magnificent adult female of the Oceanographic Institut at Monaco has as many as fourteen. The part probably played by cutaneous respiration was mentioned previously.

The cœlom is a spacious cavity lined by a pinkish-red pavement epithelium. In it lie suspended the heart and the ovary. There is no constriction separating pericardium from gonocœle. Small cœlomic outpocketings cap the ends of the branchial heart appendages. Wide cœlomiducts open anteriorly into the kidney sacs and the oviducts leave the anterior part of the cœlom close beside the cœlomiducts to open at the gill bases. From the posterior end of the cœlom a very

slender canal leads backwards to the apex of the body where it opens into a peculiar apical cœlomic organ which is lodged within the cup-shaped conus of the shell. This canal is readily traced in dissections on account of the red color of the cœlomic epithelium. The apical cœlomic organ is suspended from the margins of the shell sac by slender muscles. There can be no doubt that the canal and sac represent the siphuncular cœlom of *Spirula* and *Nautilus* (Naef, 1913), which has persisted in the vampyromorphs in spite of the disappearance of a chambered phragmocone.

Vampyromorphs are provided with moderately well chitinized jaws and a radula of relatively simple tooth pattern, not unlike that of *Sepia*. Previous investigators have not studied the salivary glands and, since the buccal mass of the specimen now under examination remains to be dissected, nothing can be said in this connection at the present time. The œsophagus can be seen through the shell sac in all specimens which are sufficiently rubbed for this structure to be exposed. It generally shows a slight ventral dilation immediately before it perforates the muscular diaphragm and occasionally this dilation is markedly distended. Serial sections reveal no histological peculiarities by which the walls of this rudimentary crop could be distinguished from those of the more anterior parts of the œsophagus. The lining is a little more folded and evidently permits distention.

The stomach is strongly muscular and opens into a cæcum of rather simple type. The rectum is without anal valves and there is no ink sac. Joubin thought that he had found an ink sac in *Retroteuthis pacifica* but it is merely the heavily pigmented rectum which can be seen vaguely through the tissues when the mantle chamber is opened.

The liver is a compact gland and the pancreas is completely incorporated in its posterior end. This is one of the few strictly octopodan characters displayed by the Vampyromorpha. In Decapoda, the pancreas is a more diffuse gland which lies loosely around the hepatic ducts.

The heart of the Vampyromorpha is also strictly octopodan; since the genital artery arises slightly dorsally near the posterior margin of the ventricle. One of the most difficult parts of the dissection has been the investigation of the venous system. It is doubtful whether a complete account will be possible. The veins are excessively thin-walled and, as in Octopoda, frequently open out into spacious sinuses. The exact extent of the venous spaces is not at present clear. A sinus surrounding the brain communicates by a long slender vein on the dorsal surface of the œsophagus with another sinus which surrounds at least part of the stomach and cæcum. The Vampyromorpha probably resemble the Cirromorpha in that the venous system is transitional between the closed system of the Decapoda and the extensive development of venous sinuses in Octopoda proper.

As in the Decapoda, an inner arm vein ring surrounds the mouth and receives brachial and interbrachial veins from the oral face of the web. A part of the evidence that the retractile filaments represent modified arms is provided by the fact that they similarly return their blood to the arm vein ring by way of superficial vessels running on their oral surfaces. There are no brachial veins on the aboral surface of the arms and there is no external arm vein ring encircling the head in front of the eyes as one usually finds in the Octopoda.

In immature animals the kidney sacs lie on either side of the rectum and have no apparent communication with each other. The excretory openings have usually been overlooked while the female pores, situated further back at the angles of the gill bases, have been mistaken for them. The relation of cœlomiducts and oviducts to the cœlom has already been explained. The ovary is a large compact gland suspended freely in the cœlom (1) by a dorsal mesentery and (2) by a special muscle originating posteriorly from the under surface of the shell sac. Only two adult female vampyromorphs have ever been captured and although the external characters have been subjected to a preliminary examination (Pickford, 1939 *a*)

their internal anatomy remains to be investigated. In both specimens the oviducal openings are surrounded by a circular sucker-like glandular structure. There are no traces of nidimental glands but the kidney sacs are remarkably enlarged and appear to communicate with each other posteriorly. Rounded bodies resembling eggs could be observed floating freely within the kidney sacs. The possibility that these may function as a brood pouch cannot be overlooked.

Even half-grown immature females can be distinguished from males by the presence of a pre-ocular pit organ which, in adults, takes the form of an ovoid sac sunk beneath the skin and opening in the center of a depressed circular area of pinkish-red skin. The histological structure and function of this organ are unknown.

Many specimens, especially in the "Dana" collections, have proved to be half-grown males. A few of these approach maturity and at least one is fully developed. In any of these specimens one finds that the penis is situated on the left side and that the large spermatophore glands hang out freely into the body cavity. This condition, not known in other living dibranchiates, is evidently primitive and was predicted by Naef.

The vampyromorph brain differs from that of the Octopoda and more nearly resembles that of Decapoda in the lack of concentration of its component ganglia. The brachial nerves arise from a separate brachial ganglion but there is much dispute as to the place of origin of the filament nerves. On account of its potential importance this point was considered both by Joubin and by Robson. Joubin found that it arose along with the other brachial nerves from what he wrongly described as the "pedal ganglion." Robson studied an already badly macerated specimen and was able to dissect the filament nerve away from the brachial ganglion and follow it back towards the pedal ganglion. In spite of the condition of the specimen his observations can be partly confirmed (Pickford, 1939 *c*). Nevertheless, in the well preserved

Bingham Oceanographic specimen, the filament nerve arises in series, along with the other arm nerves, from the brachial ganglion. Possibly the tracts continue through the ganglion into the pedo-brachial commissure, and hence could be dissected out in a macerated specimen. The matter will be settled by further investigation.

The sense organs are well developed. The ear vesicle is large, in young larvae actually larger than the eyes. The eyes are with an open pupil as in the Oigopsida. Reports of specimens with pedunculate eyes result from the fact that the entire eye bulb is frequently seen protruding out of the pupil, no doubt due to violent contractions at the time of death. The eye stalk is encircled by a pinkish-yellow skin gland which seems to have been mistaken by Chun for the white body. A white body is absent. Olfactory papillæ are situated behind the eye on either side near the angle of the mantle opening.

In conclusion, the Vampyromorpha may be defined as a new order of dibranchiate Cephalopoda in which there are five pairs of arms, the second dorsal pair being modified to form retractile filaments; in which, on the unmodified arms, the suckers are arranged in a single row, each sucker alternating with a pair of cirri; in which there is a large, uncalcified and unchambered horny gladius provided with a broad, blunt pro-ostracum and extensive lateral plates confluent with the conus vanes; in which an apical pair of slender larval fins is replaced at metamorphosis by an adult pair of more anteriorly situated paddle-shaped fins; in which the fin supports are cartilaginous thickenings in the wall of the shell sac; in which the larva possesses a forwardly projecting dorsal mantle fold that does not articulate with the back of the head and which, in the adult, becomes completely obscured by the overgrowth of subcutaneous tissues so that the mantle is completely continuous dorsally with the back of the head; in which the gills resemble those of the Decapoda in structure but possess a greatly reduced number of primary gill lamellae; in which the genital artery arises separately from the postero-

dorsal region of the ventricle and in which the heart is wholly Octopodan in character, while the venous system shows a partial development of venous sinuses; in which the digestive system is relatively simple and unspecialized, the pancreas being incorporated in the compact liver as in the Octopoda and an ink sac being absent; in which there is a spacious and unrestricted cœlom surrounding the heart, gonad and branchial heart appendages and communicating by a slender canal with a rudimentary siphuncular cœlomic sac situated in the apex of the body; in which there are two oviducts in the female, but only a single left genital duct in the male; and in which the spermatophore glands of the male are not enclosed within a genital pocket. Habitat:—The great depths of all tropical oceans.

REFERENCES

Brunnmüller, E., 1929. "Zur Genus- und Artcharakteristik von *Vampyroteuthis infernalis* Chun," in: Z. wiss. Zool., Bd. 135, p. 302.

Chun, C., 1903. "Aus den Tiefen des Weltmeeres," Jena.

Chun, C., 1915. "Cephalopoda" in: Wiss. Ergebn. Deutschen Tief—see Expedition, Bd. XVIII, t. 2.

Joubin, L., 1937. "Les Octopodes de la Croisière du 'Dana' 1921–22," in: Dana—Report No. 11. (Contains references to many of Joubin's earlier contributions.)

Naef, Ad., 1921. "Cephalopoden," in: Fauna u. Flora d. Golfes v. Neapel, 35.

Naef, Ad., 1922. "Die fossilen Tintenfische," Jena.

Pickford, G. E., 1936. "A new order of dibranchiate Cephalopods," in: Anat. Rec., 67 (1), suppl. no. 1, p. 77.

Pickford, G. E., 1938. "The Vampyromorpha—a new order of Dibranchiate Cephalopods," in: Rep. Brit. Assoc. Adv. Sci., 1938, p. 427.

Pickford, G. E., 1939*a*. "A Re-examination of the types of *Melanoteuthis lucens* Joubin," in: Bull. Inst. Oceanogr., Monaco, No. 777.

Pickford, G. E., 1939*b*. "A revision of the types of *Vampyroteuthis infernalis* Chun," in: Zool. Jahrb. Syst., Bd. 73, Heft 1, p. 47.

Pickford, G. E., 1939*c*. "On '*Melanoteuthis beebei*' Robson and the Vampyromorpha of the M. Y. 'Rosaura' Collections," in: Ann. Mag. Nat. Hist., Ser. 11, vol. iv, p. 338.

Robson, G. C., 1929. "On the rare abyssal Octopod, *Melanoteuthis beebei*," in: Proc. Zool. Soc. London, 1929, p. 607.

Robson, G. C., 1932. "A Monograph of the Recent Cephalopoda, Part II. The Octopoda," London. (Contains a complete bibliography up to 1931.)

Thiele, J., 1915. In Chun (1915).

SECTION OF ANTHROPOLOGY

April 22, 1940

Dorothy L. Keur, Hunter College, New York, N. Y.: *New Light on Navaho Origins.* (This lecture was illustrated by the use of opaque projection apparatus.)

All are agreed that the Navahos are not indigenous to the area in which they were first observed by the Spaniards. Evidence given by the linguist and the ethnologist, documents of the earliest historians, and traditions of the Puebloans and Navahos themselves, all attest the non-southwestern origin of the latter. The problem of their origin may be tersely formulated as follows: Whence and when did they come, and by what route?

The linguist has pointed out that the great Athabaskan stock, to which the Navaho language belongs, has its historical center of gravity somewhere to the north. Athabaskan is spoken not only by the Navaho and Apache of the southwest, but also by several small tribes in northern California, such as the Hupa, Tolowa, and Kato; and by the Dene tribes of western Canada, including the upper and lower Yukon, and the Mackenzie River valleys. Sapir further links the Athabaskan stock to the Haida and Tlinget, to form the Na-Dene group. On the basis of the degree of dialectic diversity, he posits the historical center of gravity for Athabaskan in the north. The southern group, by contrast, is quite homogeneous.

The historical data are significant in considering the period after the Navahos had reached their southwestern home. Escalante and Dominguez, in 1776, were the first to map the location of the Navaho. They indicate a region north and west of Zuni as "Provincia de Nabajoo." Other historic documents tell of fortified mesas, the cultivation of maize in nearby fields, and the presence of flocks of sheep. They indicate, albeit sketchily, the multiplicity and intricacy of Navaho-Pueblo relations.

History and ethnology both give evidence that the Navahos probably obtained maize, sheep, the arts of weaving and silversmithing, and quite possibly of sand painting, from the Pueblos. The hogan is distinctive to the Navaho, and possibly the cooking pots or utility ware. This pottery seems un-southwestern in tradition.

The linguist, historian and ethnologist all turn to the archaeologist for further light in the solution of Navaho origins. This is a problem demanding the direct historic approach, starting from a known and documented horizon, and pushing back to prehistoric evidence. Until very recently, the interest in Navaho archaeology has manifested itself only in observation and survey. As early as 1912, Kidder noted three settlements in the upper San Juan Valley, New Mexico, which he suggested were probably of mixed Pueblo and Navaho origin. Each settlement contained small Pueblo units and associated hogan-like structures, both of which were completely surrounded by unbroken defense walls. Presumably they were historic in time, since a "hooded" fireplace, believed to be of Spanish origin, was present in one case, and the timbers appeared to have been cut with metal axes. They strongly suggest refuge sites. In 1916, Nelson observed comparable ruin sites in the Largo Canyon. Others were reported as occurring sporadically in Carriso and Burns Canyons, and in the Puerco drainage. Stallings and Hall recently made a survey in the Gobernador region and found hogan sites associated with round towers, presumably watch towers for defense. Malcolm, in 1937, made a survey of hogan sites in the Chaco Canyon area and reported 48 sites. Farmer, 1938, located 29 sites in the Largo and Blanco Canyons, several of which were associated with two-storied, round towers. Farmer carefully excavated two of the hogans.

Our work was undertaken in the summer of 1939 in Guadalupe Canyon, 75 miles northwest of Albuquerque, New Mexico. 95 hogan sites were excavated. 72 of these occurred atop Big Bead Mesa with many other types of structure in associa-

tion. There were sweat houses, lean-tos, caches, rock shelters, dance grounds and fortifications. Nothing distinctly Puebloan in the architecture was present.

Hogans were of two types:—the forked stick and the stone circle. The former consisted of several (3–5) pinyon poles, forked at the upper end, set in the ground approximately equidistant on the circumference of the hogan floor, with forked tops interlocking; hence producing a conical framework. Other timbers were placed between to fill out the structure. The stone circle type consisted of a ring of rough boulders or a circular wall of rectangular sandstone blocks, more or less regularly coursed. Often natural outcrops or huge sandstone boulders were utilized as a section of the stone ring. It is not known exactly how such hogans were roofed, but a cribwork of small and light timbers seems a possible means. The hogans varied in diameter from 6 feet to 15 feet. After excavation, the bare hard packed floors often revealed only fireplaces. Their usual location was slightly off-center, toward the doorway. Other features occasionally present include deflectors of large stone slabs set on one side of the fireplace, mealing bins, caches, metates and manos, and a pair of small holes neatly lined with white sandstone, presumably to support loom posts.

Natural cavities, or recesses formed by overhanging sandstone ledges, were utilized as caches. In most cases, a stone wall had been constructed across the opening. Most caches were entirely empty, but one yielded over 500 corn cobs. The corn was of a variety indistinguishable from that used by the Pueblos.

Sweat houses could be recognized by tell-tale heaps of fire-reddened stones and the fireplace could usually be located from their position. The sweat houses were somewhat removed from the groups of hogans and one occurred at the foot of the main trail up the mesa. This suggests their use in purification rites and is in keeping with modern sweat-bath practises.

The two dance-grounds discovered appeared to be flat

cleared areas, roughly retangular in form, extending directly east of hogans, and lined by several fireplaces. The larger of the two measured approximately 20 by 30 feet. Ten fireplaces were located here, arranged in two rows. This would indicate the proper setting for the Navaho Night Chant.

Fortifications consisted of the crude piling of stones along natural promontories and of a spectacular great wall, erected across a narrow neck of Big Bead Mesa, cutting off a northeastern spur with 23 hogans from the remainder of the mesa. If the enemy succeeded in gaining access to the mesa top, a last stand could well be taken behind this great wall, 26 feet long and 12 feet high. On its northeast face, the wall has a setback, forming a ledge 5 feet above the ground, where guardsmen could have been stationed. Three peep holes, evenly spaced in the upper part of the wall, could have furnished them the necessary sights. At the head of the principal and only readily accessible trail up the mesa, stood a large grooved boulder, presumably another defense device. It seems plausible that the boulder, 4 feet in diameter and weighing several hundred pounds, had been encircled by a rope or thong set firmly in its groove, and thus was suspended from the top of the trail, so that it could have been sent crashing below on hostile visitors, by simply cutting the thong.

The chief means of subsistence of the inhabitants of Big Bead Mesa appears to have been maize and sheep. Bones of the latter were plentiful and also deer bones, indicating a considerable amount of hunting. Large parts of horse skeletons were excavated in the fill of the hogans. It is quite difficult, but possible, for a horse to ascend the mesa today.

Tools and implements were crude and simple. Stone work included such types as hammerstones, mauls, metates, manos, choppers, shaft straighteners, scrapers, knives and arrow heads. The latter varied considerably in form, but were no different from those described for the Pueblos. The materials out of which they had been manufactured, notably obsidian, chert and chalcedony, were conspicuous, since no source for

them was found in the vicinity. Bone work was very scarce. A few small beads occurred, a few awls and one piece of worked antler. One bone tube (swan) had been cut and notched at one end.

In addition to such ceremonial structures as the sweat-house and dance-ground mentioned above, evidence of ceremonial life was found in a small cache of sand-painting materials. Four colors were represented here. These were lumps of extremely soft and friable red and yellow sandstone, a piece of white chalky material, and large fragments of charcoal. In a deep recess under an overhanging ledge, were found two spruce poles, with side branches neatly trimmed, and bases pared to long slender points. These were regarded as very sacred by our informant. The presence of spruce was notable, since no spruce was observed now growing within a radius of 100 miles. Such pointed poles are said to be swallowed by some of the performers on the ninth night of the Mountain Chant.

The ceramics, as indicated by the sherds of both surface and fill, manifested an interesting dichotomy. There seems to be a basic distinction between cooking or utility ware, and slipped or/and painted Puebloid ware. Utility ware is characterized by round or conical bottoms, flaring rims, thin, gray to black paste, marked scoring, and frequent pitching. Infrequently, decoration is present in the form of appliqued fillets applied around the neck, and then stick marked or punctated. This general type of ware has a very widespread distribution, and, in general, might be called Woodland. Rather close analogies have been noted to the Dismal River sherds from Nebraska, and to Ute sherds found in Southern Colorado. At any rate, this ware seems un-southwestern in tradition, except for its appearance in the Gallina sites of the Chama region.

The Puebloid wares were of two types, an early "Anasazi" group, and a late, 17th to 19th century group. The former, consisting of Pueblo II to III black on white and corrugated

wares, were presumably intrusive, and might well have been pilfered from nearby Anasazi ruins. The latter type of Puebloid wares is significant in that it might furnish a clue to Navaho-Pueblo relations. The wares fall into four groups: (1) Navaho polychrome, classified as a Puebloid ware because of its obvious puebloid origin; (2) Ashiwi polychrome, of a Acoma-Laguma affiliation; (3) Puname polychrome, of a Zia-Santa Ana affiliation; and (4) Black-on-Red with Gobernador polychrome affinities. It is notable that in the case of (2) and (3), the affiliations appear to be with Keresan peoples. Such anomalies have been found, however, as a Puname polychrome bowl with a very Hopi-like, black-on-red design on the exterior; or a Puname polychrome olla, with a characteristic Jemez handle.

The Puebloid sherds fall within the 18th and early 19th centuries, according to Stallings. This is neatly confirmed by the dates obtained by Hall, through dendrochronology. These dates run from 1689 + x, and 1782 ± 1, to 1811 ± 20.

In conclusion, let us note the problems which remain quite unanswered, and which future archaeology may help to solve.

(1) At what time did the Navaho actually come into the southwest, and by what route? To answer this, we must seek earlier, and, if possible, "pure" Navaho sites, and possibly track down a trail of pot-sherds and house rings to the north.

(2) What area in the southwest did they first occupy?

(3) What was the extent and nature of Pueblo contact?

(4) How much of the present Navaho culture, if any, is of an Athabaskan base? Were they an earth-lodge dwelling, pottery-making, yet non-agricultural group, when they came?

Hence it appears that we have merely introduced the subject, by the first few probes back into the past, from the known historical and ethnological level.

NEW MEMBERS

Elected April, 1940

SUSTAINING MEMBER

Sullivan, Harry Stack, M.D., President, William Alanson White Psychiatric Foundation, Bethesda, Md.

ACTIVE MEMBERS

Archbold, Richard, Research Associate, Mammalogy, American Museum of Natural History, New York, N. Y.

Brasefield, Stanley Eugene, Ph.D., Professor, Applied Mathematics, Rutgers University, New Brunswick, N. J.

Brown, Howard W., M.D., Physician, Medical Office, Columbia University, New York, N. Y.

Buswell, Arthur M., Ph.D., Professor, Chemistry, University of Illinois; Chief, Illinois State Water Survey, Urbana, Ill.

Caldwell, Mary L., Ph.D., Assistant Professor, Chemistry, Columbia University, New York, N. Y.

Downes, Harold C., Ph.D., Instructor, Chemistry, Lafayette College, Easton, Pa.

Duggan, Walter F., M.D., Ophthalmologist; Staff Member, St. Elizabeth Hospital, Utica, N. Y.

Durant, Edward, New York, N. Y.

Ellwood, Walter B., Ph.D., Physicist, Bell Telephone Laboratories, New York, N. Y.

Epstein, Albert K., B.Sc., Chemist, President, The Emulsol Corporation, Chicago, Ill.

Feld, Jacob, C.E., Ph.D., Consulting Engineer, New York, N. Y.

Fray, Walter Wendell, M.D., Assistant Professor, Radiology, University of Rochester, Rochester, N. Y.

Goldsmith, Eli D., Ph.D., Instructor, Biology, College of the City of New York, N. Y.

Haugaard, G., C.E., Biochemistry, Physical Chemistry, Rockefeller Institute, New York, N. Y.

Miller, Rodney B., Assistant, Science Department, Newark Museum, Newark, N. J.

Mullen, James A., Ph.D., Assistant Professor, Biology, Fordham University, New York, N. Y.

Müller, Otto H., Sc.D., Research Associate, Surgery, Cornell University Medical School, New York, N. Y.

Murphy, Edward Joseph, B.Sc., Research Physicist, Bell Telephone Laboratories, New York, N. Y.

Neergaard, Charles F., A.B., Consultant, Hospital Planning and Construction, New York, N. Y.

O'Shaughnessy, Agnes Elizabeth, M.A., Consulting Psychologist, New York, N. Y.

Powell, Norman J., Ph.D., Director of Research, Civil Service Commission, New York City; Instructor, Public Personnel Administration, Brooklyn College, Brooklyn, N. Y.

Ricci, John E., Ph.D., Assistant Professor, Chemistry, New York University, New York, N. Y.

Suydam, Vernon Andrew, Ph.D., Head of Department and Professor, Physics, Beloit College, Beloit, Wis.

Tallman, Ralph C., Ph.D., Director of Research, Schieffelin & Co., New York, N. Y.

Taylor, Edward H., Ph.D., Professor, Zoology, University of Kansas, Lawrence, Kansas.

Thompson, Louis Ten Eyck, Ph.D., Principal Physicist, Naval Proving Grounds, Dahlgren, Va.

Tishler, Max, Ph.D., Research Chemist, Merck Co., Inc., Rahway, N. J.

Tomer, Stephen J., M.A., Professor, Chemistry, San Francisco Junior College, San Francisco, Cal.

Traver, Jay R., Ph.D., Instructor, Zoology, Massachusetts State College, Amherst, Mass.

Trudel, Paul J., Ph.D., Director, Laboratories, J. N. Adam Memorial Hospital, Perrysburg; Associate, Biology, University of Buffalo, Buffalo, N. Y.

ASSOCIATE MEMBERS

Anderson, O. D., Ph.D., Assistant Professor, Psychobiology, Cornell University, Ithaca, N. Y.

Beaglehole, Ernest, Ph.D., Resident Director, School of Social Studies, Santa Rosa, Cal.

Bencowitz, Isaac, Ph.D., Research Chemist, Newgulf, Texas.

Brownmiller, Lorrin T., Research Chemist, Alpha Cement Co., Easton, Pa.

Eisenmenger, Walter S., Ph.D., Research Professor, Agronomy, Massachusetts State College, Amherst, Mass.

Elliott, Myron A., Ph.D., National Bureau of Standards, Washington, D. C.

Emelianoff, Natalia V., D.Sc., Physical Chemist, Washington, D. C.

Ewing, Gifford, B.A., Oceanographer, Amenia, N. Y.

Fellows, Katharine A., M.S., Special Editor, Science, Middle English Dictionary, University of Michigan, Ann Arbor, Mich.

Fielding, William J., Psychology and Anthropology, Newark, N. J.

Flosdorf, Earl W., Ph.D., Assistant Professor, Chemistry, School of Medicine, University of Pennsylvania, Philadelphia, Pa.

Gross, Paul M., Ph.D., Professor, Chemistry, Duke University, Durham, N. C.

Hallowell, A. Irving, Ph.D., Professor, Anthropology, University of Pennsylvania, Philadelphia, Pa.

Hannum, C. A., M.S., Assistant Professor, Zoology, University of Arizona, University Station, Ariz.

Hobbs, Marcus E., Ph.D., Instructor, Chemistry, Duke University, Durham, N. C.

Johnson, William Cumming, Jr., E.E., Associate Professor, Vocational Guidance; Director of Placement, Virginia Polytechnic Institute, Blacksburg, Va.

Martin, Lawrence Forstall, Ph.D., Senior Chemical Engineer, Southern Regional Research Laboratory, U. S. Dept. Agriculture, New Orleans, La.

Mehl, John Wilbur, Ph.D., Research Associate, Instructor, Biochemical Sciences, Harvard University, Boston, Mass.

Miller, Harry F., Ph.D., Chemist, General Electric Co., Pittsfield, Mass.

Rabinowitch, Eugen I., Ph.D., Research Associate, Chemistry, Massachusetts Institute of Technology, Cambridge, Mass.

Schultz, C. Bertrand, M.Sc., Vertebrate Paleontology, Assistant Director, University of Nebraska State Museum, Lincoln, Neb.

Stickel, Paul W., M.F., Silviculturist, U. S. Forest Service, Dept. Agriculture, New Haven, Conn.

Stone, L. Joseph, Ph.D., Research Editor, Department of Child Study, Vassar College, Poughkeepsie, N. Y.

Sturtevant, Julian Munson, Ph.D., Assistant Professor, Physical-Organic Chemistry, Yale University, New Haven, Conn.

Tinker, Martin B., M.D., Surgeon, Cornell Infirmary; Ithaca Memorial Hospital, Ithaca, N. Y.

Todhunter, Elizabeth Neige, Ph.D., Associate Professor, Nutrition, State College of Washington, Pullman, Wash.

Tormey, Harold J., M.Sc., Head of Department and Professor, Chemistry, St. Bonaventure College, St. Bonaventure, N. Y.

Toro, Rafael A., M.A., Head of Department and Professor, Botany and Plant Pathology, College of Agriculture and Mechanic Arts, Mayaguez, Puerto Rico.

Tozzer, Alfred Marston, Ph.D., Professor, Anthropology, Harvard University, Cambridge, Mass.

Traum, J., D.V.M., M.S., Professor, Veterinary Science, University of California, Berkeley, Cal.

Trimble, Otis C., Ph.D., Associate Professor, Psychology, Purdue University, Lafayette, Ind.

Trivelli, A. P. H., Ch.E., Physical Chemistry, Assistant Superintendent, Emulsion Research Dept., Eastman Kodak Co., Rochester, N. Y.

Weber, Neal Albert, Ph.D., Associate Professor, Biology, University of North Dakota, North Forks, N. D.

Williams, Roger John, Ph.D., D.Sc., Professor, Chemistry, University of Texas, Austin, Texas.

STUDENT MEMBER

Schwebel, Robert L., Student, Biology, New York University, New York, N. Y.

TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 2 JUNE, 1940 No. 8

SECTION OF GEOLOGY AND MINERALOGY

MAY 6, 1940

PROFESSOR BRADFORD WILLARD, Lehigh University: *Recurrent Paleozoic Continental Facies in Pennsylvania.*[1] (This lecture was illustrated by the use of maps and charts.)

All Paleozoic systems are represented in Pennsylvania. Commencing with the Ordovician, stratigraphic units of continental origin are known to occur and to displace marine units in each system. Omitting the Juniata of the Ordovician which is non-orthodox, the continental facies are successively the Jonestown (Ordovician), Bloomsburg (Silurian), Catskill (Devonian), Pocono and Mauch Chunk (Mississippian) and the Pennsylvanian and Permian systems. The earlier continental units are dominantly red, but from the Silurian on, non-red (greenish to gray) beds increase in proportion to the red so that by the close of the Paleozoic era, practically no red remains. Throughout this red to gray shift there is an alternation between the two types. It is assumed that the red beds are all syngenetically colored through derivation from reworked red regolith. Intervals of diastrophic quiescence of the land mass, Appalachia, afforded opportunity for such

[1] This paper will be published in full in the Annals of the Academy.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 2, No. 8, June, 1940.

This publication is distributed to Members and is published monthly from November to June, inclusive, at N. Queen St. and McGovern Ave., Lancaster, Pa., by The New York Academy of Sciences, Seventy ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

Entered as second class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

regolith to accumulate. Uplift rejuvenated streams. With removal of the red cover, erosion cut away deeper, non-red, fresh rock. Such material deposited under fresh-water conditions became the non-red continental beds. Repetition of these stages is expressed in the alternation of red and non-red continental beds. The final dominance of the non-red is interpreted as an expression of the culmination of the Appalachian Revolution which marked the maximum uplift of Appalachia and its deepest erosion.

SECTION OF BIOLOGY

MAY 13, 1940

PROFESSOR ALAN BOYDEN, Rutgers University: *Serology and Animal Relationship.*[1] (This lecture was illustrated by lantern slides.)

Serology is that division of biology which deals with the nature and interactions of antigens and antibodies. Antigens are substances capable of inducing the formation of antibodies under appropriate conditions. Antibodies are substances which have the capacity of combining with and reacting upon the antigens which induced their formation and with chemically similar substances. In the present discussion the antigens used were the sterile sera of common mammals, amphibians and crustaceans. The antibodies (precipitins) were obtained by the repeated injection of particular sera into rabbits or fowls and the ultimate withdrawal of the antisera from the injected animals. These antisera were sterilized by Seitz filtration and stored in the ice box until used.

There are two great principles which apply to the comparison of the relative intensities of serological reactions:

1. An antiserum will react more strongly with the particular antigen used in its formation (homologous reaction) than with equivalent amounts of other substances (heterologous reactions).

2. The intensity of reaction between an antiserum and various antigens is proportional to the chemical similarity of the antigens tested.

These principles form the basic theory of all the applications of serological reactions, such as the precipitin test used in these studies, to the determination of animal relationship. What is done is to react each kind of antiserum, such as an antibeef serum, with beef serum (homologous reaction) and then with equivalent amounts of the sera of other related mam-

[1] Publication of the Bureau of Biological Research.

mals such as sheep, pig, horse, dog (heterologous reactions) and measure in some manner the relative intensity of reaction of each antiserum with the various antigens. Actually the results of such tests are measures of the chemical similarity of the serologically active constituents of the sera of the animals compared. This is, therefore, a literal interpretation of "blood-relationships."

Knowledge of animal relationship is of fundamental importance to zoologists. We mean by the term "animal relationship" an expression of the degree of similarity in the essential natures of the organisms compared. In so far as the characters measured are determined by heredity "animal relationship" coincides with "genetic relationship." Knowledge of such relationship is of fundamental importance to zoologists for these reasons:

1. Such knowledge is necessary for the development of a natural classification, *i.e.*, one which expresses the relative degrees of similarity in essential hereditary traits.

2. With such a classification generalizations can be drawn which are applicable to the members of natural groups and the transference of the conclusions drawn from experiments on particular species in the group to others in the same group is possible.

3. Knowledge of these natural groups can serve as a check on experimental results obtained with a particular species and as a basis for the determination of the relative importance of the experimental findings. For example, two investigators using one very similar, *i.e.*, closely related, species come to contrary conclusions as to the result of a particular kind of experiment. If both experimenters are right then the experimental findings are special cases and lack general validity. Or, either or both of the experimental results may be wrong and a repetition of the experiments is always indicated where there is lack of agreement in closely related animals.

4. Finally, knowledge of animal relationship, *i.e.*, genetic relationship, is necessary for our understanding of the nature

of evolution. The results of evolution are genetically related animals with varying degrees of similarity and differences. If we cannot ascertain which organisms are genetically related we do not know what the results of evolution have been, and, therefore, our understanding of evolutionary processes would be very inadequate.

If knowledge of animal relationship is so important, all possible means of determining such relationship should be employed. There are three chief divisions of Zoology used as bases for systematic analysis, (1) Morphology; (2) Paleontology; (3) Biochemistry. The first two of these sciences are well developed at present and have been used intensively for more than a century in systematic work. The last one and particularly that branch of biochemistry known as serology is much more recent and is, practically speaking, a twentieth-century development. In spite of long and intensive use of morphology and paleontology as bases for systematic work, we are still far from our goal of a truly natural classification and there seems indeed some confusion among taxonomists as to what the goal is and as to the general principles involved. As a matter of fact the grand object of classification everywhere is to group objects according to their likenesses. In biology the real goal of taxonomy is to group organisms in proportion to their degrees of resemblance in essential hereditary traits. It has often been stated that the goal of taxonomy is the expression of phylogenetic relationships. Actually the phylogeny is always inferred from the degree of similarity in characteristics believed to have genetic basis and a classification is not based on a phylogeny. Rather, both classification and phylogeny are developed from data regarding the essential, *i.e.*, hereditary natures of the organisms classified. It is true that we still have whole phyla of "uncertain systematic position" and nowhere have quantitative concepts been applied to systematic work with any great success. Some additional criterion for the determination of animal relationships which shall be entirely independent of morphology and paleon-

tology and which, it is hoped, will be more objective and more quantitative and less subject to convergence and environmental influence, is eminently desirable, to serve as a check on the other bases of systematic determinations.

Improved techniques for the performance of the precipitin reaction have been developed and the results of these tests examined to see whether serological comparisons can aid in the obtaining of the needed evidence. The ring test results on common mammals (beef, sheep, pig, horse, dog) gave values indicating the amount of serological similarity as obtained by testing an antibeef serum with beef 100%; with sheep 69.3%; with pig 13.2%; with dog 10.5%; with horse 9.4%. It should be pointed out that in the interpretation of these results the difference between the dog and horse values is not significant though each value is an average of several determinations. Nor may we conclude that because dog and horse are of the same apparent degree of similarity to beef that they are similar to each other. Each species must be compared with all the others by making antisera to each and testing these with all the other antigens. When this was done it was found that beef and sheep are most similar, that pig comes next and that horse and dog are least similar and about equally different from all the others. When plotted graphically 100% similarity means practical identity, *i.e.*, one locus on the family tree. Starting from the beef locus the sheep comes next with approximately 70% similarity or 30% difference which can be plotted as 30 units distance. Pig comes next with 13% similarity or 87 units distance from beef. Then follow dog and horse with 10% similarity or 90 units distance from beef. Taking all the values together a 3-dimensional expression was obtained showing beef and sheep close together, pig next and relatively distant, then dog and horse far from each other and from beef and pig.

Briefly the results roughly paralleled the systematic position of the species compared except that the horse and dog are not as distant from beef and sheep as they should be judg-

ing by the 87 unit distance between beef and pig, and by their morphological differentiation. In other words, the sera here are too conservative as between the orders Artiodactyla, Perissodactyla and Carnivora, in comparison with their differentiation within the order Artiodactyla, even though the pig does belong to a different sub-order of Artiodactyla (Suina) than the beef and sheep which are included within the sub-order Pecora.

Similar studies of common Amphibia in cooperation with G. K. Noble indicated the natural groups Caudata and Salientia as very distinct from each other and showed high similarity between *Rana pipiens* and *Rana catesbeiana*. Among the salamanders Cryptobranchus was used as a primitive genus and Necturus, Amphiuma and Siren were found to be fairly close to each other (12–22% distance) and much more distant from Cryptobranchus (83–97% distance). These data were of aid in establishing the position of Siren, which has been somewhat of a puzzle. The evidence indicated that Siren was not primitive but rather the farthest removed of the salamanders tested from primitive Cryptobranchus.

The results described above were all obtained by the use of the ring test and are, therefore, but crudely quantitative in nature. The procedure of measuring the volumes of precipitate obtained by the interaction of precipitins and various antigens was found to be too laborious for extended use. Recently an instrument for the measurement of turbid systems has been developed in our laboratories by R. L. Libby. This proves to be highly accurate and sensitive, capable of comparing the intensity of precipitin reactions in terms of readings on a galvanometer which registers the diffraction of light from turbid particles as it falls upon a photoelectric cell. It has been established by Libby that the "Photronreflectometer" readings are proportional to the amount of reacting antigen and antibody present.

With this promising instrument it is possible to distinguish between the sera of closely related animals and even to com-

pare the serological similarities of animals of known genetic relationship. For instance, a comparison of horse, mule and ass sera gave results indicating that the mule serum was between that of horse and ass and considerably nearer the horse. Antibeef serum gave heterologous reactions in the following order: bison, goat, elk, pig. A few comparisons have been completed using anti-blue crab serum which gave reactions in the following order: Callinectes, Carcinus, Cancer.

In all of the above Photronreflectometer comparisons the results are in accord with the systematic position of the species compared and with the genetic relationship where that is known. Without any further detail we may attempt a comparison of the relative advantages and disadvantages of the morphological, paleontological, and serological data upon which systematic classification may be based. Morphology has the advantage that it applies to all organisms and presents the greatest complexity of the character assemblage. On the other hand, its very complexity leads to difficulties in systematic grouping because different structural characters vary in their conservatism and it is difficult to summate the total degree of resemblance and reduce it to a simple expression. Morphological comparisons are often not suited to quantitative analysis and may be disturbed by convergence. Only paleontology can give us a rough chronology of some ancestral features, particularly those readily fossilized, *i.e.*, the hard parts of animals. It does not apply to all animals for whole phyla of existing animals such as the Ctenophora, Platyhelminthes, Nemathelminthes and major groups of other phyla are not and probably never will be adequately recorded as fossils. It is subject to the same limitations as morphology, besides its own incompleteness, but where the fossil history of hard parts is relatively complete, its evidence is decisive in phylogenetic analysis. As to serology it is better adapted to quantitative analysis, it is apparently free, or practically free, of convergence where proteins alone are concerned, it is least influenced by the environment and relatively constant during ontogeny.

These comparisons indicate that the data of these different zoological departments are in a real sense *complementary* for each one presents certain advantages over the others. On the same assumption that a more natural classification, *i.e.*, a classification which groups organisms according to their degree of similarity in essential hereditary traits, will result from the synthesis of all three types of data, we need to understand and appreciate more fully the peculiar contribution of each discipline and the possibilities of greater progress through mutual aid, toward the goal of a truly natural classification.

SECTION OF PSYCHOLOGY

May 20, 1940

Professor Wayne Dennis, University of Virginia: *Infant Reaction to Restraint: An Evaluation of Watson's Theory.*

Watson's theory of three native emotions, each with its specific stimuli, is so well-known that I shall not impose upon you to the extent of repeating it. Its neatness, its simplicity, its finality make it very satisfying. It has a brevity and conciseness which makes it as delightful as the statement that there are but six continents and three races, and it suffers from some of the same deficiencies.

The views of Watson concerning the emotions are often spoken of as if they constitute a closely knit structure. In point of fact, this is not the case. They may be subdivided into numerous theories, any one of which may be modified without greatly changing the remainder. If other native emotions were added to the list, or if some were subtracted, the general theoretical structure would not be much affected, just as one can add or subtract a continent or a race without altering one's general conception of the world or of the human race.

In this paper, I shall discuss chiefly but one of Watson's hypothesized native emotions; namely,—the rage reaction in response to restraint, but I hope that the treatment of this one response will show what is wrong with his scheme, and what will be wrong with any scheme of native emotions or native reaction tendencies which is based on the same principles.

It will be fruitful, first, to sketch the history of Watson's theory. That restraint of movement causes a decided reaction which may be called rage was first stated in 1917, in an article (29) entitled "Psychological Experimentation and Emotional Reactions." The theory is usually attributed to

Watson, although the original article was written jointly by Watson and Morgan. While Morgan assisted in the experimentation, he was not associated with the dissemination of the doctrine, and departed from it in later publications. However, for Watson, the theory of the native emotions which was first enunciated in the article cited above was not a view lightly held and easily relinquished. He presented it on many subsequent occasions. It appeared in a volume (15) entitled "Suggestions of Modern Science Concerning Education," published in 1917 and republished in 1921; in the Psychological Review, 1919 (25); in "Psychology from the Standpoint of a Behaviorist," of which the first edition appeared in 1919 (24) and the second in 1924; in the Scientific Monthly, 1921 (30); in the proceedings of the Annual Meeting of the International Kindergarten Workers, 1921 (26); in "Behaviorism," 1924 (28); and, in later editions, in "The Psychologies of 1925," (19), and in "The Psychological Care of Infant and Child," 1928 (27). The wording was practically unchanged throughout. In addition to its presentation in these more scientific works, it also found a place in popular journals. In view of this wide publication by a man who was very much in the public eye at the time, it is not surprising that Watson's theory became a matter of common knowledge nor is it surprising that it was incorporated into many textbooks in psychology and was accepted uncritically for at least a decade.

Watson's experimental contributions to the topic of native reactions may be said to have ended with his first article on the subject; that is, in 1917. All later publications were essentially restatements of his first findings, with no new factual contributions concerning infant emotional patterns. His research was interrupted by his participation in the work of the army during the World War and was ended by his departure from Johns Hopkins in 1920. In spite of his extensive writings, Watson never presented in detail the results of his infant research. He spoke of experimentation upon several hundred infants, but his reports are simply generalized

statements of his observations. This form of presentation gives an impression of stereotypy of infant reaction which he probably did not intend to convey.

The stimuli which Watson employed to restrain the infant were numerous. They included holding the head of the infant between the experimenter's hands or between pads of cotton, holding the nose, pressing the arms to the sides, and holding the legs tightly together. Watson repeatedly stated that the essential stimulus to rage was the "hampering of the infant's movements." He claimed that in no case was the pressure which was exerted sufficient to cause real pain, although this is a point concerning which only the baby could have had immediate knowledge. It is interesting to note the behaviorist taking such liberties with the infantile consciousness.

After Watson, the next experimenters who made use of similar stimuli administered to the newborn infant were Sherman and Sherman in 1925 (21). These investigators applied pressure to the chins of 83 infants. They found that diffuse arm reactions occurred; but the Shermans were more interested in the fact that in older infants the movements were better coordinated than they were in the fact that the reactions, as described by them, were somewhat similar to those which had been reported by Watson.

The later reports of Mandel Sherman (20) are widely known and frequently are cited in partial refutation of Watson's findings. Sherman, in a part of his experiments, exhibited to judges babies which had been stimulated in ways unknown to the judges. He also exhibited movies of infants reacting to stimuli which had been deleted from the films. One of his forms of stimulation consisted in holding the infant's head on the table "with fairly firm pressure." The point which was emphasized by Sherman was the fact that the observers did not designate with much correctness the causes of the reactions which they saw. This, of course, tells as much about the observers as it does about the infants. We do not know whether the reaction patterns of the infants or

the perceptual patterns of the observers were loosely organized. However, when the infants were restrained the observers almost invariably described the reaction as being of a vigorous character, and reported it as anger, fear, pain or colic so that it can be gathered that Sherman's restraint, like Watson's, resulted in pronounced reactions.

Next comes the monograph of Pratt, Nelson and Sun (18). A section of this publication deals with reaction to restraint. In one experiment the investigators "held the (infant's) arms pressed firmly against the body and held them there against whatever energy the infant would exert." However, in a tabular statement of the ensuing reactions one finds that sometimes the "arms immediately flexed to the original posture" and that sometimes the "arms did not remain at the sides of the body." One must conclude either that the infant occasionally overpowered Pratt, Nelson and Sun, or that these remarks refer only to what happened after the child was released. If that is the case, then the experimenters failed to state what happened while the infants were being held.

The only condition under which a sufficiently large number of subjects were stimulated by Pratt, Nelson and Sun was the state described as "asleep and dry." Watson, on the other hand, worked only with wide-awake babies, and it is likely that he did not intend his observations to apply to sleeping children.

In another part of their work, Pratt and his collaborators held the infant's nose between the thumb and forefinger with gentle pressure in such a way as to close off the nostrils. In some experiments this was done for 6 seconds, in others the stimulation was continued for 15 seconds. However, if the infant cried, the stimulation was immediately discontinued. In their typical style, these investigators report their results as follows:—To 234 experiments on infants who were asleep and dry, there occurred 197 movements of the extremities, 134 of drawing back of the head and 70 of arching of the spine. These experiments are often cited as being somewhat contrary

to the theory of Watson. If, however, it be conceded that Pratt, Nelson and Sun probably stimulated the subjects less vigorously than did Watson, if it be borne in mind that the condition of the infant when it was stimulated was different in the two investigations, if it be recalled that the duration of the stimulation was not the same, and finally, if one be reminded of the different manner in which the results were reported, it will be seen that there cannot be a contradiction between the two, since the second study scarcely in any sense was a repetition of Watson's. The most reasonable conclusion from Pratt, Nelson and Sun's work would seem to be that less severe stimuli cause less vigorous reactions.

Taylor's research (23) comes nearer to duplicating Watson's conditions for rage, but in this experiment, too, many of the infants were asleep. Furthermore, the stimulations, which consisted in cutting off inhalation by holding the nose between the experimenter's thumb and forefinger, or in holding the arms at the sides, lasted for only 20 seconds, whereas Watson, in personal correspondence with Taylor after Taylor's experiment was completed, suggested that a duration of one minute should have been used. In Taylor's investigation each of 40 infants was stimulated once by each of the two methods listed above. To nose stimulation, 6 subjects stiffened the body, 11 slashed with the arms, 30 abducted the head and 21 arched the back. To arm stimulation, 15 stiffened the body and 8 gave vigorous limb reactions. While Taylor concluded that he did not observe Watson's rage pattern, the correctness of this conclusion hinges upon what one means by a pattern. The responses which Taylor observed most frequently were a part of Watson's "pattern" but the pattern did not occur in its entirety at each stimulation.

I wish to report next some experiments which Mrs. Dennis and I performed upon a set of non-identical twins. Other researches upon these subjects have been described elsewhere (7, 8, 9, 11), but the following observations have not been previously reported. When the infants were about two

months old, they were presented with certain stimuli and their responses to these were recorded by motion picture camera. A series of stimuli was presented each day for 18 days. The stimulations included holding the head between the experimenter's hands, so that it could not be moved, and pressing the nose of the subject with the experimenter's forefinger. The amount of pressure involved in these two forms of stimulation was not measured objectively, but I can say that it was vigorous, since it was desired to obtain some responses to each stimulation.

In any form of experimentation one expects some variation in response from stimulation to stimulation, and this variation Watson's reports tended to ignore. In our records, the variability of the response is marked. Nevertheless, in approximately two-thirds of the stimulations, the movie records show behavior which may be characterized as struggling, thrashing about, or crying. This is not very different from Watson's so-called rage pattern.

But we not only stimulated the infants by holding the nose and by restraining the arms, but also by strong taste stimuli—a saturated salt solution, a very bitter quinine solution and by dilute citric acid. So far as we were able to determine, the limb reactions to these stimuli were not distinguishable from those which followed the stimulation by restraint. Let me call them by the neutral title of restlessness and crying. It is my belief that many strong and persistent stimuli will cause roughly the same sort of infant reaction.

Was Watson correct in assuming that it was *restraint of movement* which caused the restlessness and crying which he observed or was it caused by intense stimulation? Watson was not a gentle experimenter as anyone will attest who has seen Watson's research film which portrays his methods of infant investigation. He did not merely hold the infant's head and arms; he exerted pressure upon them. Pratt, Nelson and Sun, and Taylor, who merely restrained the movements of their subjects did not obtain such vigorous responses as did Watson.

In addition to the observations of the experimenters just mentioned, several other considerations suggest that restraint of movement of itself does not cause restlessness and crying. One of these considerations arises from the situation of the infant *in utero*. Here, if anywhere, is to be found a profound hampering of movements. The head, the arms, the legs, the trunk, all are restrained. There is no free space in which to move. The arms cannot be extended, the feet meet resistance in kicking, the head cannot be lifted from the chest. Yet the evidence indicates that the mature fetus moves only occasionally. From Watson's theory, one would expect the unborn child to be in a permanent state of violent rage. There is no indication that this is the case, and no indication that the prenatal responses which do occur are the result of restraint. Watson seems never to have faced the question as to why the infant immediately after birth should react emotionally to a condition very similar to that in which it exists for several months in relative quiescence.

If we consider the effective element in Watson's stimulation to have been the intensity of the stimulation rather than its hampering character, no such contradictions in interpretation are met. Intrauterine pressure is not great, and, since the fetus is in a fluid medium, the pressure is approximately equal upon all parts of the body. Each part is subjected only to a very gentle pressure.

No psychological theory ever revealed so clearly the need of psychology for an historical and cultural perspective as did Watson's hypothesis of a negative reaction to restraint in early infancy. Had Watson appeared upon the scene of Western civilization prior to 1750, he could not have formulated such a view, nor, in his own time, could he have done so if he had considered the methods of infant care prevalent before the modern era. The theory should have been forestalled by a knowledge of the widespread practice of swaddling and binding the infant. An adequate history of swaddling, so far as I can ascertain, has not been written. We know

that swaddling clothes were employed by the Jews, the Greeks and the Romans before the Christian era, and were used throughout the medieval period in all European countries. Swaddling did not begin to disappear in England until 1750, following a severe criticism of its detrimental effect upon infant health by an influential physician, Cadogan (3). Swaddling is practiced by the Lapps of today. I am told that it persists in some Italian peasant districts, and I have no doubt it is to be found in other parts of Europe as well. The Albanian custom is known through the study of Danziger and Frankl (6).

In its most extreme form, swaddling involved wrapping the infant round and round with long strips of cloth in a manner such that the arms were held extended along the sides and the legs were bound together. Furthermore, at times, the head was fastened to some sort of a stay in order to prevent it from wobbling. The general idea underlying swaddling practices was that of straightening the infant and of protecting him from injury. While the general nature of swaddling in Europe is clear from works of art and from casual references to it, I have nowhere been able to find a very adequate description of how it was actually done, or of how tightly the infant was bound. Salzman (19) states that the infant was able to move only its head; Crump (5) gives some description of the practice as does Buffon (2). The condition of the swaddled infant was such that, according to Watson's theory, the infant continually should have expressed rage. It has not been possible to find a single statement concerning the newborn infant's reaction to the swaddling process. Tiedeman, it is true, mentions a slight repugnance to swaddling on the part of his son, but this was when his son was several months of age. If children had uniformly cried during most of the time that they were swaddled that fact could hardly have escaped observation, nor, I think, could the practice have endured for centuries.

But we are not dependent upon European sources for information concerning reactions to swaddling. The binding of infants occurs in many parts of the world other than Europe. It was practically universal on the continent of North America at the time of its settlement by white men. The early explorers frequently mentioned the binding of the American Indian infant, probably because it differed in one or more respects from European binding. One of these differences consisted in the fact that whereas the European child was swaddled and then lain in a crib or on a bed, the swaddling of the Indian infant bound him to a rigid or semi-rigid cradleboard. The Indian infant was more restrained than was the European child. The accounts of various travelers, explorers, missionaries, do not indicate that the Indian infants objected to this treatment. In fact, many observers commented on the good behavior of infants when thus bound.

In some sections of America, many aboriginal customs still persist, and one can observe at the present time the reaction of the Indian infant to his bindings. I have been interested in making such observations among the Hopi and Navaho Indians of Arizona and New Mexico. Since these observations have been published in detail elsewhere (10, 12) they need be mentioned only briefly here. They show beyond doubt that the young Indian infant does not cry when bound, although his limb movements and bodily movements are very greatly restricted.

In the Southwestern culture area, the head of the infant remains free to move. But among some Indian groups, in the past, even the head of the infant was fixed. This condition was imposed as a part of the process of head-molding or head flattening, which was most prominent along the Northwest Coast, particularly among the Chinook. The procedure made the head completely immobile. At the same time, the limbs were encased by bindings. The flattening of the forehead, so as to make the head more or less wedge-shaped, was achieved by placing on the forehead a hard pad, which was pressed

against the forehead by means of the thongs which passed through the floor of the cradle and were tied. The thongs were kept tightened to what was considered to be just the right degree, the adjustment of them often being entrusted to skilled and experienced individuals. The resulting head shape was a mark of high social station and was considered to lend beauty. The process began shortly after birth and was continued, almost without interruption, for several months, often from 8 to 10 months. The head-molding technique has been described for various Northwest Coast groups by Kane (16), Swan (22), Cox (4), and Gunther (14). It is not followed at the present time.

Most of the writers cited above have said nothing about the behavior of the infant who is being subjected to this treatment. This, in itself, would suggest that the infant's behavior was not unusual. Bancroft (1) and Cox (4), however, stated that the infants seemed to feel no pain, and Kane (16) observed, "I have never heard the infants crying or moaning. . . ." Kane's interpretation, however, was that the infants may have been partially stupefied by the procedure. The eyes are described as bulging as a result of the intracranial pressure. The fact may be, however, that this pressure, although great, caused no pain. One can readily demonstrate to himself that pressure applied to the *forehead* does not hurt, whereas pressure applied to the sides of the head, where it was administered by Watson and others, is quite painful. The temporal regions of the head are particularly sensitive to compression.

The argument up to this point may be summarized as follows:—The newborn and slightly older infant has no unique pattern of reaction to restraint. He reacts to all strong and persistent stimuli with restlessness and crying, a crude pattern of activity which is similar for all sorts of intense and enduring stimuli. The effective aspect of the stimulations employed by Watson, Sherman, Taylor and ourselves was probably strong pressure or pain. On the other hand, methods which achieve restraint of movement without steep gradients of

pressure—such as the conditions which are found *in utero,* in swaddling, and in binding to a cradleboard—do not produce restlessness and crying. These statements, I believe, will harmonize all the seemingly contradictory evidence which has been presented.

Let us turn now to a consideration of the older infant. In Watson's theory, the negative reaction to restraint is characteristic not merely of the neonatal period but it is a permanent part of the human make-up. In "Psychological Care of Infant and Child" he stated that no amount of training will ever completely eliminate the response.

In later infancy, everyone knows that "thwarting" of various sorts will lead to something which may be called rage. The early baby biographies are full of instances of reaction to frustration. For example, it was recorded that one baby cried because he was not permitted to pull his grandfather's beard. Such stimulation is "restraint" only in a metaphorical sense. It is likely that Watson knew that such responses occurred, and his research may be interpreted as an attempt to find an instinctive basis for the responses to frustration. We have seen that he was probably wrong in tracing them back to the neonatal level. Rage, I believe, is a reaction which is not present until purposeful behavior has been developed and is foiled in attaining its end. It is unlearned in this sense, that when the child has developed a purposive sequence of behavior which can be interfered with, he will exhibit "rage" on the first occasion on which this interference occurs.

It was possible to observe early reactions to thwarting in the case of the two infant subjects previously referred to, since they lived under controlled conditions which made it possible for us to know when a particular activity first suffered interference. As soon as the infants became accustomed to playing with a rattle, which was not offered them until the eleventh month, each twin cried when her rattle was taken away. Another type of thwarting arose as follows:—When we picked up the infants, it was usually for the purpose of feeding them

or bathing them or experimenting with them. But occasionally we picked up an infant who was quiet and contented and returned her to the crib without further ministrations. In these circumstances she invariably cried. Furthermore, if we failed to experiment with either child at the usual time of day, she became restless. This may also be looked upon as a form of frustration, or at least as an interference with the usual routine.

Of course, physical restraint of movement may be employed as *one* means of frustrating the older infant. When our twin subjects were several months of age we restrained their arms by removing the arms from the sleeves of their garments, and buttoning the clothing over the arms so that they could not be employed as usual. This did not exert pressure but it made it impossible for the subjects to carry out their ordinary manual activities. This situation, of course, led to restlessness and crying.

Some Pueblo Indian infants at several months of age (10) begin to object to being placed on the cradleboard. But it is important to note that the Indian infant of this age has been off of the cradleboard each day for an interval sufficient to accustom him to manual play and to other activities. The negative reaction to the cradleboard which sometimes occurs in older infants may be due not merely to restraint but to the prevention of customary activities.

Physical restraint of movement does not *always* cause negative reactions in the older infant. In the case of the two infant subjects to which I have already referred several times, attempts were made to test reaction to restraint in the tenth month by repeating the head and nose stimulations which had been employed in the third month. No such stimulations had been presented during the interval. But, while Mrs. Dennis and I had not restrained the infants during this period, we had stimulated them in innumerable other ways. Not only had we fed, bathed, dried and clothed the subjects but we had plied them with odors, tastes, sights and sounds, as parts of

experimental tests. To most of these they had reacted favorably. In consequence, when one of us leaned over the crib and held the head of either subject very firmly between our hands, her reaction was to quietly look up and *smile*. For several minutes no negative reaction occurred. Eventually the child became tired of the stimulation, but no sooner than did we. It was sometimes necessary to hold a subject's head immobile for a full five minutes before she became restless and began to fret. I feel certain that it was discomfort rather than restraint which occasioned the final fretting.

It must be emphasized that the head and nose stimulations, in this situation, were not frustrating or thwarting in character. The infants were lying on their backs in their cribs, without toys, unoccupied. Their behavior indicated that they definitely wanted us to come near them. Any attention on our part, so long as it was not painful, they welcomed. Under the circumstances, their positive reactions to the head and nose stimulations were a form of positive social reaction. In other circumstances, holding the head or pressing on the nose might have been a pronounced form of thwarting, and might have led to different results.

Such considerations show the probable futility of attempts to test for "instincts" or native emotional stimuli after the very early months of life. Every stimulus which is presented, even if it has never before been employed, bears some relationship to the experience of the child. This is a fact which has general importance beyond the bounds of the mere topic of reaction to restraint. The child's reaction tendencies with respect to any test situation, like his reaction to restraint, can be stated, not absolutely, but only in terms of the relationship which the test situation bears to past situations and to the behavior repertoire. The child of several months of age does not react to a piece of fur, a snake, a loud noise, or a pinching of the nose, as a unique stimulus, as a thing-in-itself, but he seems to place each situation into some category of stimulation. A stimulation is a playful approach, a firm oral admin-

istration, a strange object, or a frustrating circumstance. The same absolute stimulating situation may be any one of these things depending upon the history of the child. Reaction tendencies beyond the first few months cannot be said to have absolute stimuli, such as loud noises or restraint of movement but instead they have relative stimuli, such as strange objects and frustrating situations (11). What is strange and what is frustrating necessarily depends upon the stimulation-history of the child.

It has been seen that reaction to frustration involves the prior acquisition of the response which is to be thwarted. Therefore, this reaction does not correspond to Watson's notion of an instinct; *i.e.*, a native response pattern elicited by certain native stimuli. The stimuli which will elicit rage are not determined solely by the structure of the individual but in part by the child's experience. In other words, a certain kind of learning is involved in reaction to frustration.

I would caution you, however, that this does not mean that this reaction is learned in the same sense that language is learned. In the past the gap between instinct and learning has been made too large. It has been customary simply to classify all behavioral phenomena with reference to this dichotomy, without attempting an analysis of the intermediate modes of development.

I believe a useful subclassification of the learned aspects of behavior can be attained by distinguishing between those things which the individual learns by himself without the aid or direction of others, and those things, like language, which are socially transmitted. The former, I have called autogenous, the latter, sociogenous behavior (11). It must be said that this division still is a crude one, but it is adequate for our present purposes.

Often we have been led by our terminology to place whatever is not totally instinctive into the class of socially transmitted phenomena. As we have accumulated evidence which indicates that stereotyped response patterns with predeter-

mined stimuli are fewer than we had supposed, we have put a great deal of emphasis upon the *social* determination of behavior. Today, no one will deny that a very large part of the behavior of the human *adult* is socially determined. But, in my opinion, we should not conclude that behavior during the first year of life also is socially determined. Most of the infant's repertoire, like his reaction to frustration, is attained through maturation plus the infant's individual experience, and does not require the intervention of other persons (11).

For it is true that the reaction to thwarting may come about autogenously. Our two infant subjects were under observation from the age of five weeks onward. In no sense can it be said that we trained them to cry when thwarted, or that we provided an example. Yet this response appeared without trial and error on the initial occurrence of each of several situations. In other words it was not a sociogenous response. To the contrary, the experiment proved it to be autogenous.

In closing, I may say that I have attempted to demonstrate the following points:—

1. At birth, the infant reacts with crying and restlessness to any form of intense and enduring stimulation, of which rough restraint may be one form.

2. Restraint of movement achieved without the use of intense stimulation does not cause negative reactions in the newborn.

3. At a later age, the infant will react also with crying and restlessness when some customary sequence of events meets interference.

4. Whether or not restraint of movement will cause negative reactions in the older infant depends upon whether or not the restraint interferes with customary sequences which have been built up. What is thwarting to the infant is not predetermined but depends upon post-natal events.

5. Interference with behavior sequences gives rise to negative reactions in the absence of any social transmission of this phenomenon.

Finally, I have suggested a briefer terminology, by which some of these views may be expressed as follows:—reaction to frustration is not congenital or instinctive but it is autogenous.

BIBLIOGRAPHY

1. Bancroft, H. H. The Works of Hubert Howe Bancroft. Vol. 1. San Francisco, A. L. Bancroft & Co., 1882.
2. Buffon, Count de. Natural History, General and Particular, trans. by W. Wood. London, Codell and Davies, 1812, Vol. III. See pp. 120–121.
3. Cadogan, W. An Essay upon Nursing and the Management of Children from Their Birth to Three Years of Age. London, J. Roberts, 4th ed., 1750.
4. Cox, R. Adventures on the Columbia River. New York, J. & J. Harper, 1832.
5. Crumb, L. Nursery Life 300 Years Ago. London, Geo. Routledge and Sons, 1929.
6. Danzinger, L., and L. Frankl. Zum Problem der Funktionsreifung. Z. Kinderforsch. 43: 219–254. 1934.
7. Dennis, W. An Experimental Test of Two Theories of Social Smiling. Jour. Soc. Psychol. 6: 214–223. 1935.
8. Dennis, W. The Effect of Restricted Practice upon the Reaching, Sitting and Standing of Two Infants. Jour. Genet. Psychol. 47: 17–32. 1935.
9. Dennis, W. Infant Development under Conditions of Restricted Practice and of Minimum Social Stimulation: a Preliminary Report. Jour. Genet. Psychol. 53: 149–158. 1938.
10. Dennis, W. The Hopi Child. New York, D. Appleton-Century Company, 1940.
11. Dennis, W. Infant Development under Conditions of Restricted Practice and of Minimum Social Stimulation. Genet. Psychol. Monog. In press.
12. Dennis, W. Does Culture Appreciably Affect Patterns of Infant Behavior? Jour. Soc. Psychol. In press.
13. Dennis, W., and M. G. Dennis. Cradles and Cradling Practices of the Pueblo Indians. Amer. Anthrop. 42: 107–115. 1940.
14. Gunther, E. Klallam Ethnography. Univ. Wash. Publ. Anthrop. 1: 173–314. 1927.
15. Jennings, H. S., J. B. Watson, A. Meyer, and W. I. Thomas. Suggestions of Modern Science Concerning Education. New York, Macmillan.
16. Kane, P. Wanderings of an Artist among the Indians of North America. London, Longman, Brown, Green, Longmans, and Roberts, 1859.
17. Murchison, C. (Ed.). The Psychologies of 1925. Worcester, Mass., Clark Univ. Press.
18. Pratt, K. C., A. K. Nelson and K. H. Sun. The Behavior of the Newborn Infant. Columbus, O., O. S. U. Press, 1930.
19. Salzman, L. F. English Life in the Middle Ages. London, Humphrey Milford, 1926.
20. Sherman, M. The Differentiation of Emotional Responses in Infants. I. Judgments of Emotional Responses from Motion Picture Views and from Actual Observation. Jour. Comp. Psychol., 7: 265–284. 1927.

21. Sherman, M. and I. C. Sensori-motor Responses in Infants. Jour. Comp. Psychol., 5: 53–68. 1925.
22. Swan, J. G. The Northwest Coast or Three Years' Residence in Washington Territory. Harper & Bros., New York, 1857.
23. Taylor, J. H. Innate Emotional Responses in Infants (In Studies of Infant Behavior, O. S. U. Press, 1934).
24. Watson, J. B. Psychology from the Standpoint of a Behaviorist. Phila., Lippincott, 1919.
25. Watson, J. B. A Schematic Outline of the Emotions. Psychol. Rev. 26: 165–196. 1919.
26. Watson, J. B. The Pre-kindergarten Age—a Laboratory Study. Proc. 24th Ann. Meet. Int. Kindergarten Workers, 184–206. 1921.
27. Watson, J. B. Psychological Care of Infant and Child. New York, Norton, 1928.
28. Watson, J. B. Behaviorism. New York, Norton, 1924.
29. Watson, J. B. and J. J. B. Morgan. Emotional Reactions and Psychological Experimentation. Amer. Jour. Psychol., 28: 163–174. 1917.
30. Watson, J. B., and R. A. Watson. Studies in Infant Psychology. Scient. Mo., 13: 493–515. 1921.

SECTION OF PHYSICS AND CHEMISTRY

MAY 3 AND 4, 1940

Conference on "The Primary Process in Photochemistry."

Professor W. Albert Noyes, Jr., Department of Chemistry, University of Rochester, was in charge of this meeting as Conference Chairman. The program consisted of the following papers:

"Excitation of Asymmetric Vibrations During an Electronic Transition," by Doctor E. Teller, George Washington University.

"The Results of Photosensitization Experiments with Various Metals," by Doctor E. W. R. Steacie, National Research Council of Canada.

"Primary Processes in Fluorescence and Photosensitization with Special Reference to Simple Aromatic Compounds," by Professor W. West, New York University.

"Photolysis of Metal Alkyls and Their Significance in Photoprocesses," by Professor H. S. Taylor, Princeton University.

The New York Academy of Sciences

announces the

A. Cressy Morrison Prizes for 1940

The New York Academy of Sciences announces three prizes offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes, I, II and III, all of which will be awarded in December, 1940. Prize I, of $500, will be awarded for the best paper on solar and stellar energy as defined below. Prizes II and III will be awarded for the best papers on a scientific subject included within the field of The New York Academy of Sciences and its affiliated Societies. The terms governing these competitions are detailed herewith.

The competition for Prize I is open to all. Prizes II and III are limited to members of The New York Academy of Sciences and Affiliated Societies, but non-members may become eligible by joining one of these organizations before the closing date.

Prize I

A prize of $500.00 is offered for the thesis adjudged by the Council of the Academy to be the most meritorious on the subject formerly submitted for a prize and described on March 10, 1930, as follows:

1. *Subject:* "What may be proved from our present knowledge as to the possibility or impossibility of released intra-atomic energy constituting an important source of solar and stellar energy."

2. *Memorandum of Mr. Morrison:* With the offer described on May 5th, 1924, and renewed in 1925 and 1930, Mr. Morrison invited discussion of a memorandum submitted by him, hoping that such discussion would add to our knowledge of the subject. This memorandum is again submitted, without change.

"It is accepted that atomic matter is discontinuous. It is assumed that the atom is composed of a nucleus, like a central sun, with one or more electrons which are in extremely rapid motion with orbits at relatively large distances from the nucleus; that these atoms under certain conditions are capable of absorbing energy or giving off energy, and that the electrons may be changed from an outer to an inner orbit, or vice versa; these changes taking place when the atom is absorbing or giving off energy.

"With the condensation of a nebula to a sun, the ordinary sources of energy, such as contraction, meteoric showers, chemical changes, possible radium activity, and in fact all those well-recognized sources of energy which have been proposed, should be credited with their relative importance. However, beyond these sources it is suggested that under the conditions of temperature and pressure in the interior of the sun, the complex atoms are reduced to simpler atoms or disintegrated; and that energy is thus released from the atoms and constitutes the principal source of energy of the sun. The estimated life of the sun is therefore continued for an indefinite period, far in excess of any previous calculation, by the added amount of energy contained in the normal atoms themselves. The following thesis is proposed:

> 'The principle source of energy of the sun is intra-atomic energy existing within the normal atom itself which is released from the atom under the conditions of temperature and pressure which exist in the sun.' "

So far as is known, the theory presented, concerning the principal source of energy of the sun, had not previously been announced. A knowledge of the physical characteristics of the atom has advanced rapidly and the tendency of scientific thought toward the possibility of intra-atomic energy being released under the conditions of temperature and pressure which exist within the sun has gained credence. It has become apparent that the wide dissemination of the above thesis has led to the belief in many quarters that the thesis has been sustained and it is now being quoted in apparently authoritative quarters as orthodox astronomy. This somewhat general acceptance of the thesis has not, however, been warranted either by the papers which have already been awarded the prizes or by any recognized authority. However, it is equally true that no alternative suggestion as to the principal source of energy of the sun has been accepted by astronomers or physicists nor has the thesis been shown to be untenable. For this reason, this prize is again offered in the hope that the science of physics has sufficiently advanced since the thesis was first proposed in May, 1924, to enable science to fill the gap in our knowledge as to the principal source of energy of the sun.

An A. Cressy Morrison Prize on the above subject was awarded in December, 1926, to Donald H. Menzel of Lick Observatory, University of California. The same author, in collaboration with P. B. Gerasimovic of Harvard College Observatory, also won the prize awarded in December, 1928. In 1930, the prize was awarded to Professor H. von Zeipel, of the University of Upsala, Sweden, for his paper entitled, "The Evolution and Constitution of Stars" and the most recent prize was awarded in 1938 to Hans Bethe. The first paper was published in Science, May 26, 1927, and the second in Publications of the Astronomical Society of the Pacific, for April and June, 1929. It is assumed that with the rapid progress of astrophysical investigation much light will be thrown in the immediate future on the thesis advanced which it is the object of this competition to bring before the scientific world.

Prizes II and III

SUBJECT:

Two Prizes of $200.00 each are offered for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society. These papers must embody the results of original research not previously published.

CONDITIONS:

(1) Eligibility. Prize I is open to all competitors. Prizes II and III are open to all members in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to November 1, 1940, to the Secretary of The New York Academy of Sciences at The American Museum of Natural History, Ceneral Park West at 79th Street, New York City.

(3) Papers. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication.

It is suggested that papers submitted for the prizes be accompanied by a sunmmary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the awards of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1940 by The New York Academy of Sciences." This statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

NEW MEMBERS

Elected May 6, 1940

SUSTAINING MEMBERS

Hadley, Ernest E., M.D., Psychiatrist, Washington, D. C.
Scudder, Sara A., B.A., Bacteriologist, City Hospital, Welfare Island, New York.

ACTIVE MEMBERS

Armstrong, Elizabeth J., Ph.D., Lecturer, Geology, Barnard College, New York, N. Y.

Auchincloss, Reginald, Ph.D., Research Chemist, Presbyterian Hospital, New York, N. Y.

Burton, Milton, Ph.D., Instructor, Chemistry, New York University, University Heights, New York, N. Y.

Campbell, H. Louise, Ph.D., Research Assistant, Food Chemistry, Columbia University, New York, N. Y.

Field, William L. W., A.M., Head Master, Milton Academy, Milton, Mass.

Fox, Frederick J., B.S., Consulting Engineer, New York, N. Y.

Freedman, Louis, Ph.D., Biochemist, Director of Research, U. S. Vitamin Corp., New York, N. Y.

Frieden, Alexander, Ph.D., Chemist, Technical Director, Stein-Hall & Co., Inc., New York, N. Y.

Friedman, Joseph S., Ph.D., Photographic Chemist, Technical Director, Colorite Inc., New York, N. Y.

Fromm, Erich, Ph.D., Psychologist, New York, N. Y.

Garcia-Diaz, Julio, Ph.D., Professor, Biology; Dean, College of Arts and Sciences; University of Puerto Rico, Rio Piedras, P. R.

Gelarie, Arnold J., M.D., Chief, Arthritis Clinic, Stuyvesant Polytechnic Clinic, New York, N. Y.

Germann, George B., Ph.D., Principal, St. Clair McKelway Junior High School, Brooklyn, N. Y.

Germer, Lester H., Ph.D., Physicist, Bell Telephone Laboratories, 463 West St., New York, N. Y.

Grayzel, David M., M.D., Pathologist, The Jewish Hospital of Brooklyn, Brooklyn, N. Y.

Hamilton, William J., Jr., Ph.D., Assistant Professor, Zoology, Cornell University, Ithaca, N. Y.

Hart, David, Ph.D., Assistant Professor, Chairman of Department, Chemistry, Brooklyn College, Brooklyn, N. Y.

Levine, Albert J., Ph.D., Psychologist. Guidance Department, Tilden High School, Brooklyn, N. Y.; Psychological Guidance Institute, Brooklyn, N. Y.

Park, Willard Z., Ph.D., Professor and Head of Department, Anthropology and Sociology, University of Oklahoma, Norman, Okla.

Schooley, James Plummer, Ph.D., Director, Endocrine Laboratories, Difco Laboratories, Inc., Detroit, Mich.

Smith, Karl Ulrich, Ph.D., Assistant Professor, Psychology, University of Rochester, Rochester, N. Y.

Smith, Preston R., M.A., Research Chemist, The Flintcote Co., East Rutherford, N. J.

Snow, Adolph J., Ph.D., Technical Director, Sears, Roebuck & Co., Chicago, Ill.

Spinden, Herbert Joseph, Ph.D., Curator, American Indian Art and Primitive Cultures, Brooklyn Museum, Brooklyn, N. Y.

Stanley, Wilard F., Professor, Biology; Head, Science Department, State Normal School, Fredonia, N. Y.

Steffen, Gustav I., Ph.D., Bacteriologist, Assistant Director, Bureau of Laboratories, Department of Health, New York, N. Y.

Sugiura, Kanematsu, D.M.Sc., Research Chemist, Memorial Hospital, New York, N. Y.

Switzer, H. B., B.S., Sanitarian, Good Housekeeping Bureau, New York, N. Y.

Tabor, Florence S., Ph.D., Instructor, Chemistry, Pratt Institute, Brooklyn, N. Y.

Thorn, Marvin D., B.S., F. S. Mosely & Co., New York, N. Y.

Townend, Robert V., Ph.D., Chemist, Technical Director, William Zinssner & Co., Arlington, N. J.

Treat, Asher E., B.A., Instructor, Biology, College of the City of New York, N. Y.

Wilson, Robert W., Ph.D., Curator, Vertebrate Paleontology; Instructor, Geology, University of Colorado, Boulder, Colo.

ASSOCIATE MEMBERS

Chase, Henry R., Jr., Vice-President, Southern Biological Supply Co., Inc., New Orleans, La.

Chase, Ralph E., A.M., Instructor, Anatomy, University of Oklahoma, School of Medicine, Oklahoma City, Okla.

Eastman, Irene M. H., Ph.D., Chemistry, Brainerd College, Brainerd, Minn.

Essenberg, Christine Adamson, Ph.D., Director, American School for Girls, Damascus, Syria.

Farmer, Albion C., D.V.M., Federal Veterinarian, Unadilla, N. Y.

Fisher, Mary S., Ph.D., Department of Child Study, Vassar College, Poughkeepsie, N. Y.

Fitt, Cleon T., Ph.D., Research Chemist, American Smelting & Refining Co., Salt Lake City, Utah.

Follett, Richard E., Secretary and Director, Zoological Society, Detroit, Mich.

Fulford, Margaret H., Ph.D., Instructor, Botany, University of Cincinnati, Cincinnati, Ohio.

Garber, Clark M., M.A., Educator, Lecturer and Author, Butler, Ohio.

Gershoy, Alexander, Ph.D., Professor, Botany, University of Vermont, Burlington, Vt.

Gingrich, Wendell D., Sc.D., Associate Professor, Bacteriology, University of Texas Medical School, Galveston, Texas.

Glass, L. C., M.S., Professor, Zoology, University of Idaho, Moscow, Idaho.

Godbey, Allen Howard, Ph.D., Durham, N. C.
Goethe, C. M., Biologist, Sacramento, Calif.
Haden, Russell L., M.D., Head, Medical Division, Cleveland Clinic, Cleveland, Ohio.
Hanson, Earl P., B.S., Writer, Rhinebeck, N. Y.
Hasselstrom, K. Torsten H. A., D.Sc., Director, Research, G. and A. Laboratories, Savannah, Ga.
Segerblom, Wilhelm, A.B., Former Head, Chemistry, Phillips Exeter Academy, Exeter, N. H.
Seifriz, William, Ph.D., Professor, Botany, University of Pennsylvania, Philadelphia, Pa.
Semmes, Douglas R., Ph.D., President, Sarnosa Oil Corporation, San Antonio, Texas.
Sevag, Manasseh G., Ph.D., Assistant Professor of Biochemistry, Bacteriology, University of Pennsylvania, Philadelphia, Pa.
Smith, Frank J., M.A., Associate Professor, Chemistry, University of Southern California, Los Angeles, Calif.
Smith, Susan Gower, M.A., Associate, Medicine, Duke University, School of Medicine, Durham, N. C.
Smith, Wilbur C., M.D., Head of Department, Gross Anatomy, Tulane University, School of Medicine, New Orleans, La.
Sontag, Lester W., M.D., Director, The Samuel S. Fels Research Institute, Yellow Springs, Ohio.
Spies, Tom D., M.D., Professor, Medicine, University of Cincinnati, Cincinnati, Ohio.
Stewart, Dorothy R., Ph.D., Associate Professor, Biology, Skidmore College, Saratoga Springs, N. Y.
Stone, Robert G., A.M., Librarian and Research Fellow, Blue Hill Observatory, Harvard University, Milton, Mass.
Stromsten, Frank A., D.Sc., Associate Professor, Zoology, State University of Iowa, Iowa City, Iowa.
Tait, William D., Ph.D., Professor, Psychology, McGill University, Montreal, Quebec, Canada.
Talbert, Ernest L., Ph.D., Associate Professor, Sociology, University of Cincinnati, Cincinnati, Ohio.
Tanner, Herbert G., Ph.D., Research Chemist, E. I. du Pont de Nemours and Co., Wilmington, Del.
Taylor, Hugh S., D.Sc., LL.D., Professor, Chemistry, Princeton University, Princeton, N. J.
Thies, O. J., Jr., M.A., Instructor and Assistant Professor, Chemistry, Davidson College, Davidson, N. C.
Thurstone, Louis L., Ph.D., Professor, Psychology, University of Chicago, Chicago, Ill.